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BIOGRAPHICAL MEMOIR

OF

OTHNIEL CHARLES MARSH

1831–1899

BY

CHARLES SCHUCHERT

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

OTHNIEL CHARLES MARSH[1]

1831–1899

BY CHARLES SCHUCHERT

Othniel Charles Marsh, for twelve years president of the National Academy of Sciences, was born to Caleb Marsh and Mary Gaines Peabody on October 29, 1831, in Lockport, New York, and died in New Haven, Connecticut, on March 18, 1899. One of the three founders of the science of vertebrate paleontology in America, his career furnishes an outstanding example of the indomitable spirit that drives men on to a determined goal. His motto might well have been, "What I have, I hold." He asked no quarter, and gave none. At home around a camp fire or in an army tent, formal as a presiding officer or in society, at times austere and autocratic, at others a raconteur of note, he left a lasting impression on his chosen branch of science.

Summarizing his work statistically, it may be said that between 1861 and 1899 he published about 300 papers, reports, and books. Of new genera he described 225, and of new species, 496; of new families 64, of suborders 8, of orders 19, and of subclasses 1.

Of his work on vertebrate fossils in general, Osborn says that he "carried out the most intensive field exploration known to science and published a large number of preliminary papers, which fairly revolutionized our knowledge."

ANCESTRY AND TRAINING

John Marsh of Salem, the first of his name recorded as emigrating from England to America, is believed to have reached

[1] In the preparation of this memorial, the writer has been aided greatly by the excellent sketches of Professor Marsh written by George Bird Grinnell, Charles E. Beecher, and J. L. Wortman. Still further insight into his background and character was furnished by the many family letters preserved in the archives of the Peabody Museum at Yale University, and by the twenty-six bound volumes of Marsh correspondence. The memorial has also had the benefit of criticism by Professor Richard S. Lull, who was for many years in charge of the Marsh collections at Yale. It is the writer's hope soon to expand the memorial into a book on Professor Marsh.

the Massachusetts Bay colony in the year 1634. A cordwainer by trade, he was very fortunate in his marriage, taking to wife in 1635/36 Susanna, eldest daughter of the Rev. Samuel Skelton, the latter a graduate of Cambridge and the "spiritual father" of Governor Endicott, at whose solicitation he came to Salem in 1629 and there organized the first church of the Puritans. Zachary, the eldest of John and Susanna Marsh's eleven children, in turn left a family of nine, and it is this line that has most interest in the present connection, because John Marsh, of the sixth generation from Zachary, and his wife, Mary Brown, were the parents of Caleb Marsh, born in South Danvers (now Peabody) on November 8, 1800.

On his mother's side, Professor Marsh's ancestry can be carried back much further, the name Peabody (or Pabody) reputedly originating early in the Christian era. Lieut. Francis Peabody, the first of the American family, came to New England from Hertfordshire in 1635—a "husbandman," 21 years of age. He lived first in Ipswich but in 1638 became one of the original settlers of Hampton, and moved to Topsfield in 1650. Like John Marsh, he picked a helpmate from a distinguished family, the daughter of Reginald Foster (or Forster), whose kin were honorably mentioned in "The Lay of the Last Minstrel" and in "Marmion". Thomas, their descendant of the fifth generation, born in 1762, married Judith Dodge of Rowley in 1788, and became the father of Mary Gaines Peabody, Professor Marsh's mother, on September 3, 1807.

It is interesting to note the purity of the English stock that lay back of the subject of this memorial. The families that are represented by Professor Marsh's eight greatgrandparents can each be traced back to the early days of the colonies without break, and several of them are known for many generations in England; and in none of the marriages so far traced does there appear any name indicative of other than English blood. The families were: Marsh, Brown, Foster, and Buxton, on the one side, and Peabody, Gaines, Dodge, and Spofford on the other.

The early years of the nineteenth century found the two households with which we are concerned living in the village of South Danvers. John and Mary Marsh, with their seven children,

shared a comfortable home, kept from ready money by the father's tendency to acquire still more land, but with definite educational traditions that sent the boys to nearby academies and at least two of them to college—John [2] to Harvard, Ezekiel to Bowdoin and then to Andover and Yale for theology. In the Peabody home, there was at first little surplus beyond living necessities, because the father had died by accident in 1811, leaving his widow to find food and shelter for eight children, the youngest of whom had been in the world only two years. Judith Dodge Peabody was made of stern stuff, however, and she had stalwart help in her sons, especially George, then aged sixteen, who was to have one of the most astonishing careers in American finance. By the time the younger girls were ready for schooling, George was providing the family with a comfortable living, and it was at his request that his sisters Mary and Sophronia were sent to Bradford Academy, which Caleb Marsh had also attended.

The Peabody family moved from Danvers to a neighboring town some time during this period, but Mary's friendship with Caleb Marsh reached the stage of an engagement in the early part of 1826. A letter was sent to Baltimore, asking her brother's consent to the marriage, and enclosing an extract of a letter from Caleb, which was concerned with the very necessary question of what the young couple were to live on. George Peabody agreed to give his sister as much as Caleb should receive from his father, so the two seemed assured of a comfortable amount on which to start life together.

Caleb and his family felt that his opportunities for making a living would be better in the new country to the west, and with that idea in mind the young man accompanied his brother John to Michigan. However, the farm that Caleb finally bought for his matrimonial venture was in Lockport, New York, on Chestnut Ridge, three miles east of the village and one mile from the Erie Canal, on the north side of the old Post Road to Albany; it had 114 acres of land, two "convenient houses", three large

[2] The Dr. John Marsh whose interesting story has been told by George D. Lyman in his book, *John Marsh, Pioneer,* Scribner's, 1930.

barns, and a very fine orchard, the price being $2000. Later he bought an additional 100 acres or more.

To this Lockport farm Caleb brought his young wife shortly after their marriage on April 12, 1827. No sorrow seems to have clouded their first years together except the loss of their first child at birth early in 1828, a loss soon softened by the coming of a daughter, Mary, in August 1829, and of a son, Othniel Charles, on October 29, 1831. There were temporary reverses of fortune such as fall to the farmer's lot, but no major catastrophe until August 1834, when the young mother, apparently recovering normally from the birth of her fourth child, was taken down with cholera and died within fourteen hours. Her husband, grief stricken, hurriedly sold his farm and went back with his two elder children to his old home in Danvers. In 1836 or early 1837 he was remarried, this time to Mary Lattin, daughter of a well-to-do Lockport man. He was engaged for a while in the shoe business in Haverhill, but shared the fate of many in the depression of 1837, and a few years later returned to Lockport, taking Othniel with him.

Of Othniel's early days, we have but little information. As the eldest boy, he was expected to be his father's mainstay in the farm work, but he preferred to range the countryside, hunting the small game then still abundant in the Lockport region, and becoming an expert shot with a rifle. While he and his stepmother seem to have been on friendly terms, nevertheless the new brood of children, which had increased to six by 1852, the recurring financial troubles, Caleb's inability to cope with them, and his consequent lack of equability, all helped to drive the wedge deeper between father and son. The boy went to school apparently in the winter only, but by 1848 he had acquired sufficient knowledge to allow him to attend Wilson Collegiate Institute at Wilson, New York, where his conduct and progress during the years 1848-1850 were reported by the principal to be "satisfactory". In 1850 he was a pupil in the Lockport Union School, and in that year he tried school teaching, but gave it up because of "headaches"—probably due to near-sightedness. However, he made money enough to enable him to follow a long-cherished wish and go back East; and the next year his doings

at the family homestead in South Danvers are recorded in a diary. This diary, it must be admitted, shows surprising immaturity for a young man of twenty, and yet the year 1851-1852 was the turning point in Marsh's life. Coming of age, he received a settlement, at least in part, of the property held for him since his mother's death (proceeds of the marriage dowry given her by her brother George), and with it, he decided to go to Phillips Academy at Andover, Massachusetts.

He entered the Academy before the turn of the year, and there he found the environment that brought to the fore qualities hitherto dormant. Not at once, however: the first year he did not exert himself, but the second year he settled down to work in earnest. His comparative maturity gave him an advantage over the other boys, and he found within himself a zest for learning. Even more significant, he very shortly developed a talent for leadership. He was at Andover five years, graduating valedictorian in July 1856; according to a statement quoted by Grinnell, "He had made a clean sweep of all the honors of Phillips Academy—there was no desirable honor which he did not get while there."

He had not been in Andover long when George Peabody extended him a helping hand, and in May 1856 Othniel carefully framed a letter to his uncle, expressing his gratitude for past opportunities, and asking permission to enter Yale College. The first actual meeting between the two took place later in that same year, and what George Peabody saw was evidently to his liking, for Marsh entered Yale College in September 1856.

At Yale, Marsh's progress was less brilliant than at Andover, but he was graduated in 1860 (at the age of 28) with a High Orations stand in the Classical Course, eighth in a class of 109. He was elected to Phi Beta Kappa, and he also received a Berkeley Scholarship, awarded for excellence in certain of the classics, and carrying with it the proviso that the recipient must remain at Yale as a graduate student for one to three years. Although his rank as a "Scholar of the House" was thus founded on the classics, Marsh had no intention of following these branches further. Before his graduation he had written his uncle that he wished to fit himself for "a Professorship of Nat-

ural Science in Yale or some other College", and had received the latter's consent. Certainly it was quite in keeping with George Peabody's own career to be willing to foster high ambition when he saw it!

It might be profitable at this point to consider what forces had been shaping Marsh's mind toward science. First among them was doubtless his country upbringing and his great love for the outdoors, expressed in his compositions at the Wilson Collegiate Institute. At the time when the Erie Canal was being excavated, in 1823, and when it was being widened, about 1843, the Lockport region became famous the world over for its Niagaran (Silurian) fossils, and this rich fauna attracted a retired army officer, Col. Ezekiel Jewett (1791-1877), who is said at that time to have been "unsurpassed in America as a field paleontologist." It is known that young Marsh came under the influence of Colonel Jewett some time after 1843—the colonel had a summer school in geology at Lockport in 1843-1847—and learned from him how and where to collect fossils, and especially minerals. By the time he went to Andover, in 1851, he already had a mineral collection of some dimension, and he added to it during five summer trips to Nova Scotia. An interesting item, in view of subsequent events, is the statement in his Andover diary that in April 1855 he called on Benjamin F. Mudge, then curator of the natural history society in Lynn, to see his minerals and to get help in identifying some of his own. During his first two years at Andover, he was in the English Department, which offered some courses in natural science, but with his decision to go to college, made in the spring of 1853, he had perforce to shift to the Classical Course.

After his graduation from Yale College, Marsh turned at once to science, then developing rapidly at Yale under the two Sillimans, George J. Brush, and James D. Dana; and it was under the guidance of these men, and especially of Professor Brush, that he spent two years of study in the young Sheffield Scientific School. As the second year drew to a close, he had to decide what should be the next step toward the professorship that had become his goal. Evidently the idea of study abroad had been in his mind for some time—doubtless placed there by

Brush, who had studied in Germany—for he wrote his uncle on June 9, 1862: "If the plan of completing my studies in Germany, which you once so kindly approved, still meets with your approbation, I should like to go in September next." Mr. Peabody readily assented, and letters of introduction given Marsh by Professor Dana indicate that he was planning to study particularly analytical chemistry and mineralogy.

Marsh started for Germany in November 1862, stopping in London to visit his uncle, to see the International Exhibition, and to look at some fossils in the British Museum that he wished to compare with certain vertebrate remains discovered on his second visit to Nova Scotia some years earlier, and forming the subject of his first paper on fossil vertebrates, "Description of the Remains of a New Enaliosaurian (*Eosaurus acadianus*)", published in July 1862. A copy of this paper had been sent to Sir Charles Lyell for publication in the *Proceedings* of the Geological Society of London, and it was communicated to that Society in December by Lyell himself. In the following year, Marsh was proposed for membership in this leading geological society by Lyell, a signal honor for so young and untried a worker.

Marsh matriculated at Berlin University as a student of mineralogy and chemistry under G. and H. Rose, respectively, and of microgeology under Ehrenberg. In the spring of 1863 he moved on to Heidelberg, to work under Bunsen, Blum, and Kirschoff. During this spring, also, he had a momentous meeting with his uncle while the latter was taking the "cure" at Wiesbaden. This conference was concerned not only with Marsh's hoped-for career, but with Mr. Peabody's "future plans and donations," regarding which the two had had "a long talk" in England the year before. When it was over, Marsh was able to write the elder Silliman that his uncle proposed to give Yale the sum of $100,000 (later increased to $150,000) for a museum of natural science.

After a summer spent in Switzerland, Marsh went back to Berlin in the fall of 1863, and spent the entire academic year in the study of paleontology, at Professor Dana's suggestion; by this time it had been made clear to him that a position in the

Sheffield Scientific School was a probability. The summer of 1864 he devoted to further excursions in Switzerland, and in October he entered the university at Breslau to study with Ferdinand Roemer (who had done much geologic work in Texas and Tennessee), Grube, and Goeppert, returning to Berlin in the spring.

Marsh's formal appointment to the professorship of paleontology in the Sheffield Scientific School—the first such chair in America—was made at Commencement, July 24, 1866. His connection with the Scientific School continued until 1879, when he was transferred to Yale College. According to Grinnell, "he did not wish to make his professorship a teaching one, and preferred to serve Yale without salary in order that his time might be devoted to research and exploration."

MARSH'S PERSONALITY

George Bird Grinnell, naturalist and writer, was one of Professor Marsh's students, a fellow explorer with him on two Yale expeditions in the Rocky Mountain country, and a close friend to the end of Marsh's life. Charles E. Beecher, who succeeded Marsh at Yale, worked in the same room with him for ten years as his assistant, had the full confidence of his superior, and probably understood him better than most other persons, besides knowing more of his later history. The memorials written by Grinnell (1910) and Beecher (1899)[3] form the basis for the following account, supplemented by the writer's own knowledge of Marsh, gained from daily intercourse with him during the year 1892 and occasional meetings during the last six years of his life, and from a study of the thousands of letters received by Marsh, his many notes relating to his own career, an abundance of newspaper clippings, and finally, the nearly three hundred scientific papers that he published.

Marsh's early life as a farmer's son had developed for him a strong frame and a robust body. Never seriously ill at any time in his life, we find him nowhere dwelling on the hardships of life or the dangers and fatigue of field work in his pioneer

[3] Cited, together with other sketches of Professor Marsh, at the end of the bibliography which accompanies this memoir.

days in the Rocky Mountain region. He stood about 5 feet, 10 inches, in his shoes, was stockily built, broad-shouldered, and erect. In early life he probably weighed around 160 pounds, and in later years about 175. In middle life his nose, mouth and chin were average in character, his face round, his complexion fair and of a healthy color. His hair and eyebrows were sandy in tone, with a beard tending toward red. He had widely spaced blue eyes that were somewhat nearsighted—a defect that led to his rejection by the Army in the Civil War days; he wore eyeglasses only while reading and writing, however, and none of his many portraits show him with such. His forehead was high, and a scantiness of front hair caused it to appear even unduly so; with middle life he became slightly bald. As a youth he wore a flowing mustache, but almost all his later portraits show a well dressed full beard.

Although possibly inheriting through his mother many of the traits that were to make him prominent, Marsh grew up without the softening influence that she might have exerted, and his favorite sister, Mary, died while he was in Andover. With his somewhat domineering father he did not get on well. Moreover, as he remained a bachelor, he had no family ties to hold him in check. Self-reliance he possessed to an extraordinary degree, and it naturally led to a self-centering of his life and ambitions. Out of it came, also, Beecher says,

"an absence of the complete exchange of confidence which normally exists between intimate friends. Even where perfect confidence existed, he seldom revealed more about any particular matter than seemed to him necessary or than the circumstances really demanded."

By anyone meeting Marsh for the first time, and especially anyone asking for information in his line of research, the caution of the man must have been instantly felt—possibly even a slightly suspicious attitude until he had made sure that he knew the whys and wherefores of the meeting. Although access to him was easy, the critical visitor soon saw the marked self-confidence that comes with wealth and position. It was but natural that he should be proud of his ancestry, especially of his relationship to George Peabody, who had made his career possible; and he

was very proud indeed of his unique professorship at Yale, as well as of his high standing among the leaders of science. The next things to impress the visitor were minor physical peculiarities, such as the blinking of his searching, nearsighted eyes, and the seeming impatience shown in his nervous, half-articulated "What? What's that?" On longer acquaintance one came to appreciate that his chief characteristic was his feeling that "the work of the hour is of prime importance," and that all those around him should be as interested in it as he was. His ambition to stand at the top is apparent from his Andover days onward, and once he had decided what road to follow, he never wavered in his determination to be one of the highest savants in science and to build up at Yale one of the world's largest foundations in paleontology. He fully accomplished all these wishes.

We get a good insight into Professor Marsh as he appeared to his Yale colleagues in the following excerpt from President Timothy Dwight's *Memories of Yale Life and Men:*

"In his personality, Professor Marsh was, as we may say, a man quite by himself. He was intelligent, with a manly intelligence, and a careful student, patient in his researches. But at the same time, as a collector and discoverer, he had the irrepressible zeal which is characteristic of an enthusiast. Every new thing in his own sphere of investigation which revealed itself—everything which had in it the promise of a revelation—gave him happiness and stirred him to fresh activity. He would press forward with all energy, and any needed outlay of effort or means, to secure what it might have to give him. When he had made it his own, and found it of true value, he hastened with joyful ardor to relate his good fortune to his friends, as if he had possessed himself of a hidden treasure. . . .

"In his attitude and in his manner of expressing himself, a certain formality was characteristic of him. Especially was this manifest in cases where he sought an interview with others on matters of business, or on subjects of interest with respect to his own particular work. The slight and somewhat peculiar hesitation in his utterance rendered this formality more conspicuous. I was always struck with this singularity of manner when he called upon me. . . . Whatever the object might be, the manner of the man was the same. It was as if we had been two ministers of state having little acquaintance with each other, who had met for the settlement of some great question of public concern. All was serious with a dignified solemnity, and measured with a diplomatic deliberateness. . . . Such idiosyncrasies made the man the more interesting. They certainly gave him an individuality which distinguished him from others."

Grinnell says that Marsh was a keen judge of men, could instantly select the one he felt would be of most use to him, and was seldom at fault in his estimate of character; that he was efficient and shrewd, with a touch of cunning, and an aggressive leader. Though not an easy writer, "he took great pains to express himself clearly and in correct English."

With the desire to know how Marsh appeared to his European contemporaries, the writer asked this question of Sir Arthur Smith Woodward, of London, whose father, Henry Woodward, was probably the closest of Marsh's friends on that side of the ocean, and from him received the following reply:

"Professor Marsh visited England usually in alternate years, and he had a large circle of friends both in this country and on the European continent. I first met him in 1884 in my second year as an assistant in the Geological Department of the British Museum (Nat. Hist.), and thenceforth I regarded him as one of my best friends. In 1890 I stayed with him in his 'wigwam' (as he termed it) at 360 Prospect Street, New Haven, and I was always closely associated with him during his visits to England. He was absorbed in palaeontological research; and in London he spent most of his time in studying the fragmentary fossil remains of reptiles and mammals in the British Museum for comparison with the much finer specimens which he had at his disposal in America. He visited other cities for the same purpose, and thus had much influence on the progress of vertebrate palaeontology in Europe. . . .

"Like other great men, Marsh had his failings; and close association with him soon revealed both his unrestrained jealousy and his love of popular adulation. His early rivalry with Cope and his later rivalry with Osborn were never-ending subjects of conversation. Flattering newspaper notices pleased him, and I remember he was delighted when the English journal *Punch* published a little picture of him discoursing on the newly discovered skull of *Triceratops* to the British Association at Leeds in 1890. . . .

"Marsh was a remarkably keen observer, and he was quick to see the inferences which might be drawn from the facts before him. He was also one of the foremost systematists of his time, and contributed greatly to the classification of reptiles, birds and mammals. . . . I am convinced that in all essentials Marsh's fundamental contributions to vertebrate palaeontology were his own, and stimulated by his boundless enthusiasm for our science."

In his Academy days, and at Yale College, Marsh learned to be an easy mixer, and his diary shows that this was true not

only among his classmates but also among his instructors; during his vacations he was constantly traveling about, calling on his relatives, on the curators of museums, on local collectors of minerals, and on others whom he had heard of as interested in natural history. This ease in meeting people, and especially notables, stood him in good stead in both America and Europe. The sunny side of his make-up was nearly always uppermost; in Huxley's words, he was "a wonderfully good fellow, full of fun and stories of his western adventures."

Marsh was also very fond of entertaining, and liked to give dinner parties in his finely landscaped home on Prospect Street, which was in reality but another museum in which to display his many trophies, his orchids, his paintings, and his endless examples of Japanese art. Here also he had as a guest, for several days in 1883, the great Sioux leader, Chief Red Cloud, whom he presented to several hundred of the distinguished citizens of the town.

It is a little difficult for a biographer, writing after this lapse of time, to understand fully the trail of hostility that Marsh left in many quarters. Beecher testifies that he was

"normally restive under restraint, and met all opposition with power and fearlessness. Having practically created the modern science of vertebrate paleontology in America, he resented any encroachment upon the particular fields of research in which he was engaged. This attitude frequently developed feelings of hostility in other investigators, and often alienated him from co-workers in his Department of Science."

Grinnell puts the same idea a little differently, thus:

"His fossils were priceless in his eyes, and he guarded them with extremest care. A man of less enthusiasm or more liberal mind might have turned over certain subjects to able assistants; Marsh's failure in this respect caused in several cases a rupture of friendly relations. . . . He had one or two unfortunate experiences with visitors; hence was somewhat suspicious. . . . Marsh's peculiarities were many, some of them being so marked as to give his enemies an opportunity to speak ill of him, which sometimes resulted in grave injustice."

Looking back through his career, Marsh appears to the writer to have been a sort of Jack the Giant Killer, for he was forever attacking errors, humbugs, and impostors. He began this in

1861, when he exploded the Nova Scotians' hopes for their gold mines, and in 1862 he corrected no less a savant than Louis Agassiz in regard to a certain feature in *Eosaurus*. Six years later, on his first trip to the West, certain so-called "human" remains found nearly 70 feet beneath the surface in western Nebraska were shown by him to belong to three-toed horses and associated animals; and huge footprints supposed to have been left by giants in ancient Nevada, turned out, under his searching examination in 1883, to be those of a well-known species of extinct ground sloth.

The richest exposure in which he figured was undoubtedly that of the Cardiff Giant in 1869. This was "a gypsum man, ten and a half feet long, nude, virile and unabashed," dug up in the dark of an October night in Onondaga County, New York, and widely shown at fifty cents a head. State Geologist James Hall, inspecting it undisturbed for "a full quarter of an hour," publicly stated it to be "the most remarkable object yet brought to light in this country and although perhaps not dating back to the Stone Age, . . . nevertheless deserving the attention of archeologists." The perpetrators of the hoax were getting rich, when Professor Marsh, suspicious of this gypsum giant from his native state, unearthed its real history with the help of a man from Iowa who, dissatisfied with his share of the profits, declared that he "got up" the giant from a block of Iowa gypsum. and that it was then shipped to Cardiff, New York, hauled by night to its burying place, and "resuscitated with full attention to all necessary details." After his inspection of the giant at Syracuse, during which he made sure that it was composed of gypsum, a substance that is soluble in water and therefore could not have retained, after burial, the polished surface that the statue displayed, Marsh wrote a letter to a newspaper friend exposing all that he had learned, and thus exploded the most uproarious hoax ever "launched upon the credulity of a humbug-loving people" (Clarke, *Life of James Hall,* 1921).

One of the two hardest battles of Marsh's life was his struggle in 1875 with the unscrupulous politicians of the "Indian Ring" at Washington, which started when he sent to President Grant, following a promise made to Chief Red Cloud, a printed

exposé of disreputable frauds that were being practiced upon the Indians at the Red Cloud Agency in Nebraska; and which ended with the cleaning out of the Agency and added the final straw to a load of evidence that had long been accumulating against certain members of the Interior Department. Many of the partisan papers of the time showered the professor with scurrilous language, but so far as Marsh was concerned these attacks had the same effect as water on a duck's back, and in no wise turned him from his determination "to clear out the varmints."

The most protracted and bitter fight of Marsh's life, that against his great rival, Professor Edward Drinker Cope of Philadelphia, broke out in 1873, when he began to point out in print the latter's attempt at antedating papers containing descriptions of new forms of vertebrates. Years of acrimony between the two climaxed on January 12, 1890, when Cope, through a newspaper writer in the New York *Herald*, burst forth with a full-page charge against the doings of Marsh and of Major J. W. Powell, Director of the United States Geological Survey, which was met a week later by a long blast of counter-accusation from Marsh. Of this exchange, Osborn says, in his life of Cope (1931):

> "Cope attacked after a truly Celtic fashion, hitting out blindly right and left with little or no precaution for guarding the rear. . . . Marsh's reply was thoroughly of a cold-blooded Teutonic, or Nordic type, very dignified and, under the cover of wounded silence, reluctantly breaking the silence of years."

During the two closing decades of the last century, nearly all the vertebrate paleontologists in the United States became partisans in this warfare, either openly or otherwise. Reverberations of the quarrel reached the Senate in 1892, when the Geological Survey was under fire because of Major Powell's advanced ideas about irrigation; and it thus became one of the factors that led to the 30 per cent cut in the Survey's annual appropriations (50 per cent in paleontology), and to the consequent discharge of many geologists, aside from Marsh, who had had nothing whatever to do with the controversies.

MARSH AS A COLLECTOR

According to Beecher, it was as a collector that Marsh was seen at his best, and the collections that he amassed during the last thirty years of his life

"form a lasting monument to his perseverance and foresight. . . . He not only had the means and the inclination, but entered every field of acquisition with the dominating ambition to obtain everything there was in it, and leave not a single scrap behind. Every avenue of approach was made use of, and cost was often a secondary consideration. The nine-tenths, when attained, were only an additional stimulus for securing the remaining one-tenth."

This preëminence of Marsh as a collector went beyond the amount of material that he acquired, because the improvement that he made in the technique of collecting was of equal importance. In 1892, when the writer was at work as a preparator of invertebrate fossils in the Yale Museum, he saw masses of rock and bone coming in from the field daily, bandaged and held together by gunny sacking that had been cut in strips, saturated with plaster of paris, wrapped over the fossils before they were lifted out of the ground and again on the under side when they could be turned over. He saw these packages, small and large, opened little by little by the skilful "bone-setters", with no disturbance of the much fractured specimens. As each part was opened, thin shellac or liquid glue was poured into all the crevices, or the parts were lifted one after another and reset in plaster of paris, so that when they dried out, the pieces were held firmly in place. He was fascinated by this resurrection of ancient bones and their preservation in all their structural glory for the edification of paleontologists. Having heard that Marsh was the inventor of the process, the writer asked him one day how this came about. Marsh replied that he got the idea from having seen medical men set broken bones in splints and hold them together with strips of cloth soaked in plaster of paris. This may be true, but rumor has it that the first step in this method was invented by S. W. Williston. In the late summer of 1877, he and M. P. Felch were trying to take up a badly fractured and much weathered *Diplodocus*, and finally, in de-

spair, Williston wrote Marsh on September 21: "Will it do to paste strips of strong paper on fractured bones before removing? . . . These strips are put on with ordinary flour-paste and can be removed I think easily." There is still extant in the Peabody Museum a bone bandaged in this way by Williston and Mudge in 1877. This seems to indicate that the former was the initiator of the bandaging method, and it would be but a step from strips of paper with flour paste to the more secure strips of sacking soaked in liquid plaster of paris. Marsh may have suggested the improvement; in any event, by 1880 this surgical device was in use by all his collectors.

At the time when he began to collect vertebrates, Marsh went on to say, the material that had been described was nearly always of a fragmentary nature, and usually consisted of teeth, broken jaws, and other isolated bones. The truth of this statement is evident at once to anyone who looks through the early publications. As Marsh said, this was the age when the usual method was to drive the pick under the bone, pull it up, rake all the pieces together and dump them into a sack, with the pious hope that the preparators would be able to fit the puzzle together. How much better Marsh and his "bone-hunters" came to do their collecting may be seen from a perusal of the plates in, for example, his monographs on the toothed birds (1880), the Dinocerata (1885), and the dinosaurs (1896), and in his many papers that give lifelike restorations of extinct reptiles and mammals.

John Bell Hatcher, the most extensive collector of Marsh's many field men, speaks of "Marsh's well known aversion to dealing with fragmentary or relatively unimportant material and seeking after the 'choicest plums' as he used frequently to express it." In another place, he adds:

> "Where a generation ago the extinct vertebrate life of America was but poorly represented in our museums by imperfect series of teeth and isolated bones, we are now able to study many of these extinct animals from more or less complete skeletons. For these improved conditions we are mainly indebted to the late Professor Marsh, either directly by reason of the vast collections acquired by him, or indirectly through the improved laboratory and field methods developed by him and his assistants."

The steady stream of vertebrate fossils that poured from the West into the Yale Museum during the years 1870–1873 came as the result of a brilliant idea on the part of Marsh—brilliantly planned and brilliantly executed—in short, the organization of four expeditions into the western territories, the personnel of which was made up mainly of recent graduates or undergraduates of Yale College and the Sheffield Scientific School, who not only did their share of the actual collecting, but paid most of the expenses of the trips. A fifth expedition, in 1874, though rich in yield, was made up of tested collectors and army men. After that date, Marsh did most of his collecting through individual paid collectors and their assistants, directed by correspondence and by occasional trips to the field.

Marsh himself said that at one time or another between 1870 and 1898, and for short or long periods of service, he employed "several hundred" helpers of various types. Included in this category were soldier escorts and their officers and guides, cooks, teamsters, "bone-diggers" and their assistants, and also his laboratory aids. There is no record of those who were thus employed for short times, but more than one hundred persons are represented either by letters or by names in the accession and account books. Of these, at least fifty were employed before 1882, and the remainder during Marsh's connection with the United States Geological Survey; and of the latter, thirty-five were collectors on the federal payroll. The collector *par excellence* was J. B. Hatcher; other efficient ones were B. F. Mudge, Arthur Lakes, M. P. Felch, W. H. Reed, S. W. Williston, Fred Brown, E. Kennedy, D. Baldwin, Gus Craven, Leander S. Davis, Sam Smith, and H. T. Martin. In the years between 1870 and 1892 the vertebrate fossils arrived at New Haven in an endless stream of boxes and packages of all sizes, and at times in carload lots, the accession books recording about 3000 shipments.

The use of his personal fortune and of the funds put at his disposal by the United States Geological Survey made it possible for Marsh to bring together what was probably the greatest collection of Mesozoic and Cenozoic vertebrate skeletons ever amassed by one individual. Cost was never his first considera-

tion—he was after big results. He told the writer that his pet specimen, the giant sauropod dinosaur *Brontosaurus excelsus,* had cost him $20,000, and that he had spent on his collections as a whole not less than $200,000 of his own money. In addition, he spent for the United States Geological Survey about $150,000.

With regard to the improvements in collecting technique brought about by Marsh, we have the testimony of J. L. Wortman, who was in a position to note the changes in methods, since he was long assistant to Cope and later a member of Osborn's large staff in the American Museum of Natural History. In his memorial of Marsh, he says:

> "The record of his discoveries is one of almost continual triumph in the bringing to light of new and strange forms of life that had inhabited the western hemisphere in the distant past. . . . The methods of collecting and preparing these fossils for study and exhibition which he has introduced in the course of his long experience form the basis very largely of all similar work in almost every palaeontological laboratory of the world, and it is a matter of common remark that nearly all the noted collectors and preparateurs have received their training under his immediate influence."

Of Marsh's vertebrate collection, presented by him to Yale University in 1898, Wortman said in 1900 that it was

> "without doubt the finest and most complete of any in the world, and, when properly installed and exhibited, will make a monument in every way worthy of the greatness of the man who dedicated his life and his fortune to its formation. The influence of his work for advancement in this department of knowledge has probably no equal in any country."

At the same time that Marsh was building up his great collection of fossil vertebrates, he was bringing together, from all parts of the world, skeletons of recent animals to be used for comparative studies. The collection of recent osteological material thus assembled was in Marsh's time one of the most complete in America.

MARSH'S CONTRIBUTIONS TO THE EVOLUTION THEORY

Darwin's epochal work, *The Origin of Species,* did not appear until November 1859, and, since it is well known that Professor

Dana was a creationist until long after that date, it is more than probable that Marsh's attention was not directed favorably toward evolution until his student days in Germany. That he was thinking about it, however, is evident from his paper of 1862 on *Eosaurus,* in which he says that, inasmuch as these bones occur in Paleozoic strata,

> "they add another to the arguments that have been brought against the so called 'Development Theory'; and they show with how great caution we should receive the assertions, so frequently and confidently made on negative evidence alone, of the exact date of the creation or destruction of any form of animal or vegetable life."

Marsh made his first visit to England in the fall of this same year, and at this time or shortly thereafter he met Charles Lyell and Thomas Huxley, the latter of whom, he tells us, was "a guide, philosopher, and friend, almost from the time I made choice of science as my life work". We also know that as early as 1865 he had been at the country home of Darwin. In any event, by 1874 he was an out-and-out evolutionist, and in his memorable Nashville address of 1877 he states that evolution is the "key to the mysteries of past life on the earth," and that "to doubt evolution is to doubt science, and science is only another name for truth". In his address of the following year, he adds that Darwin's *Origin of Species* had in two decades "changed the whole course of scientific thought . . . Darwin spoke the magic word—'Natural Selection,' and a new epoch in science began."

The demonstration of the truth of the evolutionary theory can come only through the study of fossils, and Marsh's well preserved and carefully collected material played a large part in the establishment of the hypothesis. It was, indeed, his specimens from the region to the east of the Rocky Mountains, where there exists an unrivaled record of dinosaurs, birds, and mammals, that helped to take the entire question of evolution out of the realm of hypothesis and to demonstrate that it is a living truth.

Marsh's first major contribution to the evidence for the evolution theory was his discovery of birds which, by their possession of teeth and other reptilian characters, proved the

genetic relationship between these two groups of animals that had been foreshadowed by Huxley. It was, however, his magnificent collection of fossil horses, and his accurate and careful tracing, from these, of the progress of the horses through geologic time, that tended to give him a greater reputation than any of his other discoveries.

It has been well said that the living horse is probably the most perfect organic machine for swift running so far developed, and that it displays throughout its organization a most exact and finished adaptation to this purpose. It has, however, taken Nature about fifty million years to perfect this mechanism. The evolution of the superfamily Equoidea "affords the best known illustration of the doctrine of evolution by means of natural selection and the adaptation of a race of animals to its environment" (W. D. Matthew 1913). This evolution begins in the Rocky Mountain country during the Age of Mammals (Cenozoic) in *Eohippus (=Hyracotherium),* only 11 inches tall, and proceeds by various genetic lines through 15 genera and some 215 different forms into the *Equus* of today.

Even previous to 1865, no fewer than ten kinds of fossil American horses had been described. Nevertheless, as Marsh tells us in his Nashville address:

"I heard a world renowned Professor of Zoology [in Germany] gravely inform his pupils that the horse was a gift of the Old World to the New, and was entirely unknown in America until introduced by the Spaniards. After the lecture I asked him whether no earlier remains of horses had been found on this continent and was told in reply that the reports were too unsatisfactory to be presented as facts in science. This remark led me, on my return, to examine the subject myself, and I have unearthed [many] species of the horse tribe . . . and it is now, I think, generally admitted that America is, after all, the true home of the horse."

In his paper on the fossil horses of America, appearing in 1874, Marsh said:

"The large number of equine mammals now known from the Tertiary deposits of this country, and their regular distribution through the subdivisions of this formation, afford a good opportunity to ascertain the probable lineal descent of the modern horse. The American representative of the latter is the extinct *Eq. fraternus* Leidy, a species almost, if not entirely identical with the old world *Eq. caballus* Linn., to which

our recent horse belongs. Huxley has traced successfully the later genealogy of the horse through European extinct forms [1870], but the line in America was probably a more direct one, and the record is more complete. Taking, then, as the extremes of a series, *Orohippus agilis* Marsh, from the Eocene [Bridger] and *Eq. fraternus* Leidy, from the Quaternary [Pleistocene] intermediate forms may be intercalated with considerable certainty from the thirty or more well marked species that lived in the intervening periods. The natural line of descent would seem to be through the following genera: *Orohippus,* of the Eocene; *Miohippus* and *Anchitherium* of the [Oligocene and] Miocene; *Anchippus, Hipparion, Protohippus* and *Pliohippus,* of the Pliocene; and *Equus*" of the Pleistocene and Recent.

"The most marked changes undergone by the successive equine genera are as follows: 1st, increase in size; 2nd, increase in speed, through concentration of limb bones; 3d, elongation of head and neck, and modifications of skull. The increase in size is remarkable. The Eocene *Orohippus* was about the size of a fox. *Miohippus* and *Anchitherium,* from the Miocene [Oligocene] were about as large as a sheep; *Hipparion* and *Pliohippus,* of the Pliocene, equalled the ass in height; while the size of the Quaternary *Equus* was fully up to that of the modern horse.

"The ancient *Orohippus* had all four digits of the manus well developed. In *Miohippus* . . . the fifth toe has disappeared, or is only represented by a rudiment, and the limb is supported by the second, third, and fourth, the middle one being the largest. *Hipparion* . . . still has three digits, but the third is much stouter, and the outer ones have ceased to be of use, as they do not touch the ground. In *Equus,* the last of the series, the lateral hoofs are gone, and the digits themselves are represented only by the rudimentary splint bones. The middle, or third, digit supports the limb, and its size has increased accordingly. The corresponding changes in the posterior limb of these genera are very similar, but not so manifest. . . . This reduction in the number of toes may, perhaps, have been due to elevation of the region inhabited, which gradually led the animals to live on higher ground, instead of the soft lowlands where a polydactyl foot would be an advantage."

Between 1868 and 1892 Marsh issued thirteen papers relating to fossil horses. His genera, arranged in the order of their description, are: *Orohippus* (1872), *Miohippus* and *Pliohippus* (1874), *Mesohippus* (1875), *Eohippus* (1876 = *Hyracotherium* Owen), *Epihippus* (1878), and *Helohippus* (1892, a synonym of *Orohippus,* as is *Orotherium* 1872).

By 1876, Marsh knew at least thirty kinds of American horses, and most of these he showed Huxley on the occasion of the latter's visit to America. The great scientist was met

by Marsh on his arrival in New York and stayed a week in New Haven as his guest, examining Marsh's general collection, which he wished to see before delivering his course of lectures. Marsh said of this circumstance later:

"One of Huxley's lectures in New York was to be on the genealogy of the horse, a subject which he had already written about, based entirely upon European specimens. My own explorations had led me to conclusions quite different from his, and my specimens seemed to me to prove conclusively that the horse originated in the New World and not in the Old, and that its genealogy must be worked out here. With some hesitation, I laid the whole matter frankly before Huxley, and he spent nearly two days going over my specimens with me, and testing each point I made. He then informed me that all this was new to him, and that my facts demonstrated the evolution of the horse beyond question, and for the first time indicated the direct line of descent of an existing animal. With the generosity of true greatness, he gave up his own opinions in the face of new truth, and took my conclusions as the basis of his famous New York lecture on the horse."

If more striking testimony to the value of Marsh's contributions to the evolution theory is needed, it is at hand in a holograph letter treasured in the Peabody Museum archives, which reads:

"Down, Kent, August 31, 1880.

"My dear Prof. Marsh,—

"I received some time ago your very kind note of July 28th, and yesterday the magnificent volume [Odontornithes]. I have looked with renewed admiration at the plates, and will soon read the text. Your work on these old birds and on the many fossil animals of N. America has afforded the best support to the theory of evolution which has appeared within the last 20 years.[4] . . .

"With cordial thanks, believe me,

"Yours very sincerely,
(Signed) CHARLES DARWIN."

MARSH AND STRATIGRAPHY

When Marsh was receiving his training in geology in the late fifties and early sixties, the stratigraphy of the marine formations was as yet very much generalized, and of the freshwater deposits there was hardly any known chronology at all.

[4] In other words, since the publication of *The Origin of Species* in 1859.

The time had not arrived when lake and river deposits could be distinguished clearly from those of the seas. Besides, there was little information that was reliable about the genetic sequence of the land plants and the animals found fossil in continental strata. It was but natural, therefore, that Marsh, while a student in Germany, should have been greatly influenced by the dictum of Professor Goeppert of Breslau, who told his classes "to doubt the value of fossil plants as indices of the past history of the world" (Marsh 1898), and that he should unfortunately have retained this opinion throughout his career. It was this prejudice that led him late in life into the grave error of thinking that continental deposits of Jurassic age are present throughout the Atlantic Coastal Plain from Martha's Vineyard and Block Island south into Virginia, despite the fact that the paleobotanists of the United States Geological Survey, on the basis of large floras described by Fontaine and Ward, had shown these deposits of the Potomac group to be of Lower Cretaceous age.

When Marsh began his researches on the Tertiary deposits of the Great Plains east of the Rocky Mountains, it was the general belief that these strata had accumulated in vast lake basins. The first edition of Dana's *Manual of Geology* (1862) held that these formations were of lacustrine and brackish-water origin, laid down in bodies of water that had been of long endurance and that had covered great areas with thick deposits. These erroneous ideas about lake deposits did not begin to break down until the last decade of the nineteenth century. Therefore in Marsh's papers treating of the Cenozoic stratigraphy of the Great Plains we read of "ancient lake basins," and that "the existence of several large fresh water lakes in the Rocky Mountains region, during Tertiary time, is now well established." The lakes, he said, were of Eocene (Wasatch, Bridger, Uinta, Green River, etc.), Miocene = Oligocene, and Pliocene ages. Even as late as 1885, in the monograph of the Dinocerata, he says that these animals are "found in a single Eocene lake basin in Wyoming. . . . This lake basin . . . slowly filled up with sediment, but remained a lake so long that the deposits formed in it, during Eocene time, reached a vertical thickness of more than a mile." That rivers in a semiarid climate lay down their

sediments over widespread flood plains did not come to be general knowledge among stratigraphers until the present century.

In none of Marsh's publications do we find that he ever actually measured or described in detail a stratigraphic sequence. He was content to note the formation in which the fossils occurred and rarely did he mention the subdivision of the formation. He left to the geologist all this most desirable stratigraphic detail, as well as the naming of the formations, since it was his ambition to develop only the biologic sequence which would show the direction evolution had taken. As a rule, his named zones were very broadly conceived.

In his description of new species of fossil vertebrates, Marsh rarely gave exact information as to their geologic level or detailed geographic position, his usual citation being "Upper Eocene of Wyoming," "the Coryphodon beds, or lowest Eocene of Wyoming," "the Miocene of New Jersey," "the Pliocene of Idaho," "the Ceratops beds or the Laramie of Wyoming," "the Atlantosaurus beds of the upper Jurassic in Colorado," and so on. For this lack of detail he was severely criticized by his colleagues, and the statement was often made verbally that his reason was that he did not want others to go to his collecting places and get specimens of his species. In his monograph on birds with teeth, he is more specific than usual as to the geologic horizon; these remarkable Kansas birds, he said, occur "in the Middle Cretaceous [which] corresponds to the strata named by the writer the 'Pteranodon beds' . . . included in subdivision number three in Meek and Hayden's section."

As we have seen, Marsh very rarely attempted stratigraphy from the viewpoint of geology. On the other hand, he was a good biostratigrapher from the standpoint of evolution as seen in the succession of vertebrate life. He was quite correct in holding that vertebrate fossils are of the greatest value in determining the stratigraphic sequence of fresh-water deposits. We may illustrate his principle by the following quotation regarding the evolution of the horse, taken from his paper of 1898:

"Near the base of the Eocene the genus *Eohippus* is found, representing the oldest known member of the horse tribe. Higher up in the Eocene *Orohippus* occurs, and still higher comes *Epihippus*, near the top of the

Eocene. Again through the Miocene more genera of horses, *Mesohippus, Miohippus,* and others, follow in succession, and the line still continues in the Pliocene, when the modern genus *Equus* makes its appearance. Throughout this entire series, definite horizons may be marked by the genera, and even by the species of these equine mammals, as there is a change from one stage to the other, both in the teeth and feet, so that every experienced paleontologist can distinguish even fragments of these remains, and thus identify the zones in which they occur."

Accordingly, it was Marsh's aim to name the successive vertebrate life zones after "the largest and most dominant vertebrate form which characterized them." By this method he had by 1896 named sixteen vertebrate life zones above the Paleozoic, as follows: Triassic, 1; Jurassic, 3 (Hallopus, Baptanodon, and Atlantosaurus); Cretaceous, 2 (Pteranodon and Ceratops); Eocene, 4 (Coryphodon, Heliobatis, Dinoceras, Diplacodon); Oligocene, 3 (Brontotherium, Oreodon, Miohippus); Pliocene, 2 (Pliohippus and Equus); Pleistocene, 1 (Bos). Many of these post-Triassic life-zone terms are still in use for the Rocky Mountain region. Osborn in 1929 proposed a different scheme for the same region, with sixteen life zones.

MARSH AND THE UNITED STATES GEOLOGICAL SURVEY

In view of Marsh's spectacular success in his earlier years as a collector of vertebrate fossils, and the rapidity with which he produced published results, it was only natural, when the reorganized and combined geological and geographical surveys were placed under the direction of Major Powell, that the latter should turn to Marsh for paleontological help. Moreover, through his Yale influence, through his wide personal acquaintance with important men, and through his long connection with the National Academy of Sciences, Marsh could also be very helpful to the Survey in easing the annual appropriations bill through Congress. According to Beecher:

"After repeated solicitations and with promises of material aid in the way of publication and collections, Marsh in 1882 accepted the appointment of Vertebrate Paleontologist to the United States Geological Survey.

This position he held to the time of his death, although the field work for the survey was terminated in 1892. His connection with the Survey gave him increased facilities for publication and for prosecuting explorations in the West."

During the ten years that Marsh was chief of the federal section of Vertebrate Paleontology, the Powell Survey was liberal in allotments for his work, and he was given about $15,000 each year to pay salaries for himself and his numerous assistants—collectors (about 35), preparators (9), scientific aids (8), and artists—and for field and laboratory expenses, including large freight bills.

The monograph of the Dinocerata was published by the Survey in 1886, and those of the vertebrates of the Denver Basin and on the dinosaurs of North America in 1896, but none of the five other monographs projected was completed. The material for them had been collected, most of the plates (about 215) and many text figures had been made, and preliminary descriptions of the genera and species had been published, but the final detailed accounts and the philosophical and phylogenetic problems were left largely untouched. As Beecher said, Marsh "planned his life work on the basis that immortality is here and not in the hereafter. It seemed difficult for him to realize the limitations of human existence and worldly accomplishment."

The material results of the ten years' service given to the federal Survey by Marsh and his staff amounted to seven carloads of vertebrate fossils. Shipments totaling two carloads (255 large boxes) were sent to the United States National Museum in 1886, 1891, 1896, and 1898. The final sending, made in 1899 after Marsh's death, filled five freight cars (529 boxes). Walcott, referring to the cessation of Marsh's relations with the Survey, said in 1900 that the value of these collections

"will be upwards of $150,000. . . . The transfer of these great collections to Washington without the loss of any material, either through imperfect recording or through misunderstanding as to the ownership of specimens, reflects the greatest credit on the business-like methods and the integrity of Professor Marsh."

PRESIDENT MARSH OF THE NATIONAL ACADEMY OF SCIENCES

Professor Marsh was greatly elated over his election to membership in the National Academy of Sciences at the April meeting of 1874 in Philadelphia, which was announced to him in a congratulatory telegram by John Strong Newberry. He was then forty-three years old, in the eighth year of his professorship at Yale; and the number of his papers describing striking new discoveries in the fossil fields of the West was just short of fifty. The geologists in the Academy at that time were a strong and influential group, including as they did Cope, Dana, Guyot, James Hall, Hayden, E. W. Hilgard, Hunt, Leidy, Lesley, Lesquereux, Meek, Newberry, Pumpelly, W. B. Rogers, and Worthen. Prior to his election, he had made one personal appearance before the Academy, by invitation, when he read a paper at the Northampton meeting in September 1868 on "human" bones found in a well in Nebraska, which had turned out to be those of an upper Miocene (Loup Fork) horse and other contemporary mammals. In the spring following his election he read his second paper, on "Size of the Brain of Extinct Mammals," and in his twenty-five years of membership, he read in all eleven papers before the Academy.

Marsh became an officer in the Academy in 1878, when he was elected vice-president. Alexander Bache, the founder-president, elected in 1863, had not lived to complete his six-year term, and Joseph Henry, who had succeeded Dana as vice-president in 1866, served as acting president until 1868, when he in turn was elected president, and was re-elected in 1874. Following Henry as vice-president had been William Chauvenet (1868-1870) and Wolcott Gibbs (1872-April 1878). At the spring meeting of 1878, Gibbs gave way to Marsh as vice-president, and on May 13, 1878, President Henry died and Marsh thus became the Academy's acting president four years after his election to membership. He held this office until the spring meeting of 1879 when, with the election of W. B. Rogers to the presidency, he resumed his status as vice-president. Curiously, however, the third president, like the other two, was not destined

to fill out his elected term. Rogers died in 1882, and once more Marsh was acting president. In April 1883, Wolcott Gibbs was elected to succeed Rogers, but as he could not serve, the presidency was given to Marsh. As a result of this odd chain of circumstances, Marsh was no stranger to the Academy's administrative duties when he became its presiding officer. That he took his duties seriously is evident when he tells us that in his seventeen years of administrative service he did not miss a single stated meeting of the forty called, nor was he absent from any of the meetings of committees of which he was a member. No other member of the Academy, either before or since his time, had as long or as strenuous a service as its presiding officer.

In 1881, the Academy had been in existence for eighteen years, and during this time no fewer than 649 papers had been read at the scientific sessions. Of these papers, only five had been published by the Academy, and President Rogers felt that the organization had not received the recognition by the scientific world that would have come if the papers of each year had been published promptly by the Academy. He proposed that they should be brought together and transmitted with the annual report to Congress, but nothing came of this move. During Marsh's presidency, the plan of issuing a volume of papers each year or two became fairly well established, six volumes (in eight parts) appearing up to 1895; and during his term there was no lapse in the publication of the annual reports.

As a presiding officer, Marsh exercised the same amount of care that he bestowed on his private affairs, and he was an active and efficient leader. Testimony as to this is included in a statement from Professor Russell H. Chittenden, a member of the Academy since 1890, who, at the writer's request, wrote down the following recollections:

"As president of the Academy during a period of twelve years, 1883–1895, Professor Marsh through his strong personality exerted an influence on the meetings of the Academy which resulted in a dignified formality in keeping with its high standing. Somewhat stern in appearance, rather punctilious in intercourse with his associates, and with a stiffness of bearing frequently misunderstood, Marsh nevertheless possessed an innate courtesy and kindness of heart which softened his

apparent hardness and made him a friend and colleague to be respected and admired.

"His interest in the Academy was deep and sincere. By many means, both direct and indirect, he sought to promote its standing with the administration at Washington, even suggesting to the President specific ways in which the Academy could be of service to the Government. Whenever possible at the annual meeting in Washington he arranged for a formal call of the Academy on the President at the White House, this at a time when there was laxity in this custom. In these and other ways he used his office to enhance the standing of the Academy as an efficient servant of the Government of the United States."

Between 1881 and 1883, Marsh had much to do with placing the administration of the Academy's trust funds on a secure basis. As he said in his address to the Academy on April 19, 1889:

"The Academy may justly congratulate itself on the possession of its trust funds for the promotion of original research in science. Three of these, for discoveries in astronomy alone, are recent gifts to the Academy, and already the Draper, Smith, and Watson gold medals, the first fruits of these donations, have been awarded, and promise to do much to encourage future study. The acquisition of these gifts made it necessary for the Academy to secure from Congress the authority to receive and hold trust funds in aid of scientific investigations, and this was accomplished in June 1884."

The first award of a medal took place in 1886, when Samuel P. Langley was given the Henry Draper medal. In the following year the Watson medal was awarded to B. A. Gould. The J. Lawrence Smith medal was presented for the first time on the evening of April 18, 1888, in the lecture room of the U. S. National Museum, the recipient being Professor H. A. Newton. This meeting was made still more memorable by the presentation of the second Draper medal to Professor Edward C. Pickering. In Marsh's time ten medals in all were given.

In 1883, the Academy began the practice of sending delegates to other learned societies and to universities, both in this country and in Europe, Marsh being delegated in 1892 to represent the Academy at the tercentenary of the University of Dublin.

As is well known, the one thing that sets the National Academy apart from other scientific organizations in America is its rela-

tion to the Government. The Academy's historian, F. W. True, states this in the Jubilee volume of 1913 in these words:

"Other scientific organizations were founded whose membership was drawn from all parts of the country, whose scope covered all branches of scientific research, and whose transactions reflected credit on their membership and on American science, but none could claim recognition as the scientific adviser to the Government."

The Academy was founded for this purpose, and its constitution was framed with this in view. Nevertheless, in the fifteen years previous to Marsh's administration, although committees had been called on twenty-seven times by the various Government bureaus for reports on technical matters, Congress itself had turned to the Academy but twice, once in 1869–1870 when the Academy was directed to draw up plans for the scientific operations of the *Polaris* expedition to the Arctic, and once in connection with the Transit of Venus Commission in 1874. During Marsh's official terms (vice president or acting president 1876-1883, and president 1883-1895), the Academy was asked for advice by Congress three times, indirectly by Congress once, and by the departments thirteen times. The work of all these committees is described in the Jubilee volume. From Marsh's time up to the Jubilee year (1896-1913), there were but five calls, a falling off of direct service that is ascribed by True to "the increase of large scientific organizations in the country, the growth of public opinion relative to scientific matters of more or less practical importance, and the development of the scientific bureaus of the Government."

Regarding this relationship of the Academy to the Government, Marsh said in 1889:

"The question has arisen, shall the Academy, in addition to the duty of giving advice when asked, volunteer its advice to the Government? Members of the Academy have urged this course at various times in the past, and during the present session the question came up again for decision. My own opinion on this subject, after careful consideration, is against such action. The Academy stands in a confidential relation to the Government, as its scientific adviser, and in my judgment it would lose both influence and dignity by offering its advice unasked.

"In appointing the committees on the part of the Academy, I informed them that the proper province of the National Academy is not merely to

make a technical examination in any case, but especially to bring out the scientific principles involved in the investigation, as a basis for future use."

Probably the most important, and certainly the most controversial, affair that came before the Academy in Marsh's period of administration was the reorganization of the various geological surveys, which was referred to the Academy by an act of Congress approved June 20, 1878, under the title, "On a Plan for Surveying and Mapping the Territories of the United States". This was concerned with the relative merits of military or civil control of public enterprises centering around the surveys of the public domain, and the discussion of it, begun as early as 1869, had become animated, acrid, and widespread. The Academy's solution of the problem was to have far-reaching results, not only for American geology in general, but for Marsh himself, whom that curious chain of circumstances hitherto noted had brought to the acting presidency of the Academy at this particular time.

The state of affairs that led up to this request by Congress has been described by George P. Merrill, in *The First One Hundred Years of American Geology* (1924), thus:

"The period of the Civil War had brought to light a considerable number of men for whom the piping times of peace, even when varied by Indian outbreaks in the West, afforded insufficient opportunities. They were men in whom the times had developed a power of organization and command. They were, moreover, men of great physical and moral courage. It was but natural, therefore, particularly when the necessity for military routes in the West and public land questions were taken into consideration, that such should turn their attention toward western exploration. . . . Willing workers were abundant and Congress not difficult to persuade into granting the necessary funds. Hence expedition after expedition was organized and sent out, some purely military, some military and geographic, with geology only incidental, and others for the avowed purpose of geological and natural history research."

As a result of this condition, there were functioning in 1874 six separate surveys of the western territories—two geological surveys under the Engineer Corps of the Army and two under the Department of the Interior, a land-parcelling survey under the latter department, and the United States Coast and Geodetic

Survey under the Treasury Department—all with little or no attempt at mutual collaboration, more or less overlapping, and consequent wasteful expenditure of time and of public funds.

The discussion finally became so acute that Congress decided reform must be brought about, and set it in motion with the following act:

"And the National Academy of Sciences is hereby required at their next meeting to take into consideration the method and expenses of conducting all surveys of a scientific character under the War or Interior Department, and the surveys of the Land Office, and to report to Congress as soon thereafter as may be practicable, a plan for surveying and mapping the Territories of the United States on such general system as will, in their judgment, secure the best results at the least possible cost; and also to recommend to Congress a suitable plan for the publication and distribution of reports, maps, and documents, and other results of the said surveys."

When this act was approved on June 20, 1878, Marsh was in Europe. Upon his return in August, after consulting members of the Academy Council and others, he at once set about fulfilling the wish of Congress. In his own words, as given in his annual report for that year:

"I was required to appoint a special committee to consider the subject. The report of the committee, when completed, could in accordance with the constitution of the Academy . . . be transmitted directly to the government, and afterward to the Academy at its next stated session. Inasmuch, however, as the subject to be considered was of great importance, I thought it better to have the report submitted first to the Academy before transmission to Congress.

"In the appointment of this special committee it was obvious that I could not properly select as members any of those who had taken part in the controversy between the then existing government surveys; which contention, it was said, had resulted in the passage of the law for the proposed reorganization. Again, the subjects to be considered by the committee pertained to mensuration, geology, and natural history, and I therefore selected those who were familiar with these branches of science."

The committee appointed by Marsh was made up of the following Academicians: James D. Dana, William B. Rogers, J. S. Newberry, W. P. Trowbridge, Simon Newcomb, and Alexander Agassiz. Their appointment, True says,

"led to a protest by General Humphreys, Chief of Engineers, who asserted that 'a properly constituted committee should have had among its members

those officers in the Government service whose duties consisted in part or in whole in making geodetic, topographic, or other scientific surveys in the different departments of the government.'"

Marsh then went on to say:

"As the surveys under the War Department and the Interior Department were the special subjects for investigation, I addressed letters to the Secretary of War and the Secretary of the Interior informing them that a committee of the Academy had been appointed to consider the matter, and requested any information as to their plans or wishes in regard to the scientific surveys under their departments they might think proper to lay before the Academy. In reply, the Secretary of War sent a communication from the acting Chief of Engineers of the Army, and the Secretary of the Interior sent reports from the Commissioner of the General Land Office, from Prof. F. V. Hayden, and from Maj. J. W. Powell, all of which were carefully considered by the committee."

The committee deliberated some three months, then handed in a report of about 2000 words which was brought before a special meeting at the autumn session of the Academy in New York City, November 6, 1878, "and after a full discussion of three hours was adopted with only a single dissenting vote". Acting President Marsh then transmitted the report to the President of the Senate and to the Speaker of the House of Representatives.

The report recommended the recombination of the various surveys into three: 1. Coast and Geodetic Survey, "whose function will embrace all questions of position and mensuration"; 2. U. S. Geological Survey, to determine "all questions relating to the geological structure and natural resources of the public domain"; 3. Land Office, to control "the disposition and sale of the public lands": all three organizations to be within the Department of the Interior.

When the report was printed, the chief opposition to it came, as was to be expected, from the War Department, and especially from General Humphreys, Chief of Engineers, but despite this the House Committee on Appropriations incorporated the whole plan proposed by the Academy in a bill (House Res. 6140) which was duly reported to Congress. The final action by Congress, however, accepted only that portion of the plan relating

to the establishment of a single geological survey under the Department of the Interior, and appointed a commission to consider the codification of laws relating to the survey and disposition of the public domain, leaving the matter of the mensuration surveys for the present in abeyance.

Another important recommendation of this report, adopted by Congress, was that

> "All collections of rocks, minerals, soils, fossils, and objects of natural history, archaeology, and ethnology, made by the Coast and Interior Survey, the Geological Survey, or by any other parties for the Government of the United States, when no longer needed for investigations in progress shall be deposited in the National Museum."

This provision was added by Marsh.

Among the reports made by Academy committees to various departmental bureaus during Marsh's terms of service, one of the most discussed was that on glucose, presented in 1882 by Remsen, Chandler, and Barker, which covered 77 printed pages. With this report Marsh had nothing to do directly, but he was in frequent correspondence with the committee during its deliberations.

Marsh's presidency of the Academy came to an end in April 1895, when he refused re-election for a third term. In his farewell address, on April 19, he said in part:

> "In conclusion, allow me to congratulate the Academy on the substantial progress it has already made, the sure foundation on which it now stands in its relations to the Government, and its high position in the ranks of the scientific societies of the world. . . . I am especially grateful for the unanimous vote of thanks by which you have set the seal of your approval on my services as vice-president and president of the Academy during the last seventeen years."

It was Marsh's wish that "the influence of the Academy should be scrupulously reserved for the promotion of noble ends." At his death in 1899, it was found that he had bequeathed to the Academy in his will the sum of ten thousand dollars "for promoting original research in the natural sciences," a sum that was later increased to twenty thousand dollars by the trustee of his estate.

MARSH'S WORK IN VERTEBRATE PALEONTOLOGY

The Astonishing Dinosaurs

Of all land animals that ever lived, none was more remarkable than the dinosaurs, and they were certainly the most wonderful creatures discovered in Marsh's time. They were the masters of the warmer parts of the continents during the Mesozoic era, and for about one hundred and thirty million years they were the rulers of all life. The Age of Reptiles saw their rise, culmination, decline, and extinction.

In general form, dinosaurs were more like mammals than any other class of animals, not sprawling as do most reptiles, but standing well up on their legs; nevertheless, they were the most specialized of all reptiles. Most significant was the smallness of their brains. It mattered not whether the head in these giants was a foot or eight feet in length, the sensory center weighed but a few ounces, or at most two pounds, in bodies that in some forms weighed as much as forty tons. Truly, the long Age of Reptiles was characterized by low mentality and brute strength.

Isolated bones and partial skeletons of carnivorous dinosaurs are now known to have been found as early as 1820 in the Upper Triassic strata near East Windsor, Connecticut; and in 1824 one of the duck-billed dinosaurs, *Iguanodon*, was described from the late Jurassic deposits of England, although the relationships were then unknown. Curiously, the numerous so-called "bird tracks" of the Connecticut Valley did not attract much attention until 1835, and it was many years later before these highly varied foot impressions were proved to be the tracks of dinosaurs. By 1841, enough different kinds of dinosaur remains were known to show the leading British anatomist, Sir Richard Owen, that these animals were different from all other known reptiles, and he proposed the name Dinosauria ("terrible reptiles") for them, regarding them as an order.

In this country, bones and teeth of small sauropod dinosaurs were found in 1858 in the Lower Cretaceous near Bladensburg, Maryland, and a single tooth of a different type was found

about this same time by Hayden in the Judith River region of Montana. Still later, remains of *Iguanodon*-like dinosaurs were recovered in the marl pits of New Jersey farmers and elsewhere along the Atlantic border.

The possibility of the Great Plains as a major source for dinosaur remains may have suggested itself to Marsh on his first transcontinental trip, in 1868, when he was shown a sauropod bone found near Lake Como, Wyoming; but at that time he was apparently too much engrossed in other things to follow up the discovery. Late in 1870, in the marine strata of the Upper Cretaceous of western Kansas, he found the greater part of a small dinosaur skeleton that he named *Hadrosaurus* (now *Claosaurus*) *agilis*. It was of the same general type as those from the New Jersey marl pits, but only one or two other specimens of this species have been found since. A few years later, Cope brought back from the Judith River region of Montana a long series of dinosaur teeth (21 species), indicating that dinosaurs had been present there in abundance, and that some of them had reached considerable size. From that time on, Marsh was on the alert for dinosaurs, and he kept himself posted as to new finds through the newspapers of the time and through correspondence with military officers and scouts stationed at the frontier posts east of the Rocky Mountains. Partly as the result of this vigilance, there were two periods in his career when the dinosaurs of the Great Plains were on the march to New Haven in carload lots. The first of these "migrations" was of late Jurassic forms, and it began in 1877 and lasted strongly until 1886; the other, of late Cretaceous forms, began in 1888 and continued until 1892. These discoveries were so remarkable that the story may be told in some detail.

Late in the spring of 1877 Marsh received a most welcome letter from Arthur Lakes, an Englishman teaching school at Denver, stating that he and Captain H. C. Beckwith, U. S. N., were digging out, at Morrison, Colorado, the bones of a gigantic animal which they wished to sell. As Cope had also been informed of this find, Marsh rushed one of his experienced collectors, Professor B. F. Mudge, to Lake's quarry, and bought all the bones (including even the few remains that had been sent

to Cope for inspection), announcing in July 1877 the discovery of "a new and gigantic dinosaur", which he named *Titanosaurus* (later *Atlantosaurus) montanus*. This striking new discovery was widely heralded in the newspapers of Denver and elsewhere, and other collectors were quickly at work. Mudge was sent to Canyon City to inspect another promising locality, and after his report, Marsh transferred Samuel W. Williston, one of Mudge's most alert students, from western Kansas to Colorado. Williston went to work with a will on September 21. It was only a short time, however, before he wrote Marsh that Cope's man, O. W. Lucas, was getting "by far the best lot of fossils," and that another man was out prospecting all the time for Cope. Young and impatient, Williston also set out to find "better bones," and by October 27 had gone as far north as Morrison. This battle of prospectors continued until Williston was ordered by telegram to go at once to Como, Wyoming, to look at still a third "bone-yard". From this place he wrote Marsh on November 14 that he had struck pay dirt with a vengeance, that well preserved bones could be had there "by the ton. . . . Canyon City and Morrison are simply nowhere in comparison with this locality. . . . I shall commence work . . . about 250 yards from the northwest shore of Como Lake."

The finding of these many dinosaur localities in Wyoming and Colorado finally led to the employment by Marsh of dozens of "bone-diggers", and during the years 1877 to 1886, Marsh and the United States Geological Survey spent there upward of $10,000 a year. In the course of these ten years about 134 small packages, mainly with mammal teeth, and at least 480 large boxes of dinosaur bones came from Como alone; in addition, Marsh received about 270 boxes of dinosaur bones from Canyon City, and 230 from Morrison. All in all, the late Jurassic formations—Marsh's Atlantosaurus beds—yielded him not less than 1115 boxes, large and small, of dinosaur remains. Out of this material he described 21 new genera and 41 new species, truly the richest harvest of dinosaurs ever garnered by a single paleontologist. Among them were the largest members of the order, *Brontosaurus* and *Diplodocus*, the carnivorous horned

Ceratosaurus, and also the most bizarre of dinosaurs, *Stegosaurus.*

The swamp-living sauropods, ponderous quadrupedal dinosaurs of world-wide distribution in late Jurassic time, included the largest land animals that ever lived, in fact, "greater than was supposed possible in an animal that lived and moved upon the land". One of the mightiest of these, the "thunder saurian," *Brontosaurus excelsus,* was described by Marsh from the most perfect sauropod skeleton ever dug up, found by Reed near Como. When the bones were discovered, Marsh states, the huge skeleton "lay nearly in the position in which the bones would naturally fall after death." In August 1883 he presented a lifelike restoration of this skeleton, the first to be published of any dinosaur, and this illustration, together with many other drawings based on his work, has gone into many textbooks. The *Brontosaurus* skeleton, as now mounted in the Peabody Museum of Yale University, is 67 feet long and 16 feet high at the hips, and the animal is estimated to have weighed nearly 30 tons in the flesh. The head is less than 2 feet long, "smaller in proportion to the body than in any reptile hitherto known," and the astonishingly small brain weighs less than one pound, while the enlarged neural ganglion in the sacrum is about three times as large. An Indian elephant ("Rya"), by contrast, had nearly 11 pounds of brain to 4 tons of body weight. A slenderer and lighter sauropod associated with *Brontosaurus* was *Diplodocus,* which had a length of nearly 80 feet to the end of its long whiplike tail.

The overspecialized stegosaurs of the late Jurassic of Wyoming and Colorado, first described by Marsh in 1877, were provided with a mighty armor composed of huge plates and long spikes, which Marsh placed in a single row down the back and tail. This curious armature, Marsh says, "could not have been anticipated and would hardly have been credited had not the plates themselves been found in position." Now, however, nearly all vertebrate paleontologists agree that these plates were arranged in two alternating rows along either side of the dorsal processes of the vertebrae, while the tail spikes were opposite, in pairs. These plates and spikes appear to have been for

defense against the terrible carnivores of the time. *Stegosaurus* was more than 18 feet in length and about 10 feet tall; the beaked head was only about 17 inches in length, with a brain 5 inches long and weighing 2½ ounces or less, to control 10 tons of animated flesh.

Marsh's second period of dinosaurian discovery was concerned with the horned ceratopsians, whose evolution was recorded in about 3500 feet of late Cretaceous strata.

The Ceratopsia were again huge and ponderous dinosaurs, ranging up to 23 feet or more in length and up to 8 feet in height, with skulls from 4 to 8 feet long. The brain in these huge heads was not over 6 inches long and "smaller in proportion to the entire skull than in any known vertebrate." The jaws were provided with a sharp cutting beak, and in *Triceratops* there was a strong horn on the nose, a pair of very large pointed horns on the top of the head, and a row of sharp projections around the margin of the posterior crest. This huge, expanded crest, which overshadowed the back of the skull and neck, was evidently of secondary growth, a practical necessity for the protection of the neck and even more so for the attachment of the powerful ligaments and muscles that supported the great head.

The region from which nearly all the best ceratopsians come is an area about 15 by 35 miles in the east-central part of Converse [now Niobrara] County, Wyoming. In this area Hatcher, with the help of O. A. Peterson, W. H. Utterback, A. L. Sullins, W. H. Burwell, and Charles E. Beecher, worked for most of four years, shipping to Marsh more than 300 large boxes, containing 31 "big skulls" and several fairly complete skeletons of horned dinosaurs, besides much other material among which were more than 5000 small mammal teeth and jaws. The largest ceratopsian skull shipped was No. 24, which, with its box, weighed 6850 pounds. The amassing of this great amount of material was, as Osborn says, one of the greatest achievements of Hatcher's remarkable life.

In the revision of the Ceratopsia made by Hatcher and Lull in 1907, we learn that of the 13 genera proposed by Marsh, Cope, Lambe, and Lull, but 6 were left in good standing within the

group, Marsh losing 2, Cope 4, and Lambe 1. Lull's memoir of 1933 recognizes 6 genera, 3 of which belong to Marsh.

In addition to the ceratopsians, Converse County yielded other dinosaurs, duck-billed and ostrich-like forms, both bipedal running types. The duck-bills (*Claosaurus* = "trachodonts") were characteristic animals of late Cretaceous time the world over, and lived along the shores of rivers and lakes. The front of the mouth had no teeth but the jaws had a horny cropping beak as in birds. Marsh's best material in this group came from Wyoming, where two entire skeletons were found, each with a length of about 30 feet and a height of 15 feet.

Marsh's work on the Dinosauria is recorded in 55 papers and books. These were issued between 1872 and 1899, and most of the results of the first 50 papers are summarized in his quarto monographs, *The Dinosaurs of North America* (1896) and *Vertebrate Fossils of the Denver Basin* (1896). In these publications Marsh named and described 80 new species (2 lost to Leidy),[5] and these he classified in 34 new genera, of which 27 are still in use (1 each lost to Leidy, Cope, and Johnston, and 4 to himself), in 7 new suborders, and 3 new orders—surely an astonishing record for one man to make in less than thirty years. Even more valuable than the descriptions is the perfection of much of the material left to posterity, since among the several hundred individuals studied by Marsh many are represented by almost complete skeletons. All this material is now in the United States National Museum, or in the Peabody Museum of Yale University.

In 1895, Marsh was not disposed to accept the view that the Dinosauria belong to two or more distinct groups, each of independent origin, because of "the very limited information we now have in regard to so many dinosaurs known only from fragmentary remains." The tendency among more recent authors, however, is to abandon the term Dinosauria, since it surely includes reptiles of two phylogenetic lines.

[5] The taxonomy followed throughout this paper is that used by O. P. Hay in his catalogues of fossil vertebrates, with one or two exceptions.

Flying Reptiles

Late in November 1870, after an especially successful day's fossil hunting in the chalk bluffs of the Smoky Hill River in western Kansas, Marsh was returning to camp with his soldier escort, when he espied from the saddle a fossil bone. The lateness of the day made it impossible for him to explore the rock further, but he saw that the bone "was hollow, about six inches long and one inch in diameter, with one end perfect and containing a peculiar joint that I had never seen before." Placing the bone in his "softest pocket," he re-examined it in camp and thought it might prove to be the tibia of a gigantic bird. Back in New Haven, with figures of other material for comparison, he saw that the bone was from the wing finger of a pterodactyl, the first remnant of these flying reptiles to be found in America, but of a much larger species than any known European form. He at once asked himself how great must have been the wing expanse of this animal when alive, and concluded that it "would be about twenty feet . . . truly a gigantic dragon even in this country of big things."

In June 1871, Marsh hurried back to the Smoky Hill River country and to the spot where he had found the bone the previous autumn. Dismounting, he found the impression of the very bone he had collected, "and following it up with great care, I obtained the upper end of the same bone." To his great joy, further digging turned up another joint which fitted onto the first one, and directly he "uncovered still another bone, and at last the whole series that supported the gigantic wing of the ancient dragon." These he measured roughly and determined that "this first found American dragon was fully as large as my fancy had painted him."

As all pterodactyls known up to 1876 were provided with teeth, Professor Marsh must have been greatly surprised to learn that his Kansas forms had none at all. In view of the fact that they showed characters so widely different from all other forms in the Old World, he proposed a new order for the American types this same year, calling it Pteranodontia, and the family, Pteranodontidae, from the typical genus *Pteranodon*. The nearly

perfect skull and jaws of *P. longiceps*, he says, "are more like those of birds than of any known reptiles," and the head of *P. ingens* was no less than 4 feet long. The tail was slender and short, and the posterior limbs, though small, were well developed.

In the decade beginning with 1871 Marsh described and named, of the order Pterosauria, 3 new genera (1 a synonym), and 8 new species (1 preoccupied) from the Cretaceous of Kansas, and 1 new genus and 1 new species from the Jurassic of Wyoming. After 1876, so many extraordinary fossils crowded in upon him that he never was able to write a detailed account of his pterodactyls. This was well done later on, however, by Williston, in eleven papers published between 1891 and 1911, and by George F. Eaton in his *Osteology of Pteranodon*, 1910. From the latter memoir we learn that the spread of wing in the usual run of pteranodons was between 11 and 16 feet, but that the largest, *P. ingens*, shows a breadth of 22 feet, 3 inches, and single large bones indicate a probable maximum of 26 feet, 9 inches. The bones are a marvel of lightness, and Williston estimates that the entire animal, when alive, did not weigh more than 30 pounds. This combination of great wing expanse and extreme lightness of body was of much interest to Secretary Langley of the Smithsonian Institution in his pioneer aviation studies, and it was at his request that F. A. Lucas, of the Museum staff, made a reconstruction of *Pteranodon*.

In addition to his American finds, Marsh also secured one of the most perfect pterodactyls ever unearthed in Europe. This was found in 1873 by Martin Krauss in the late Jurassic lithographic limestone of Eichstätt, Bavaria, and there was great rivalry as to who was to have the specimen, which was of the long-tailed type with an abundance of teeth. Marsh cabled his friend, Prof. H. B. Geinitz, to secure this remarkable fossil for him, paying about $1000 for it and other Solenhofen fossils. It is now one of the gems of the Peabody Museum. It belongs to the genus *Rhamphorhynchus*, and Marsh gave it the specific name *phyllurus*. The bones of the skeleton, he says,

"are nearly all in position, and those of both wings show very perfect impressions of *volant membranes* still attached to them. Moreover, the

extremity of the long tail supported a separate vertical membrane, which was evidently used as a rudder in flight."

Marine Lizards (Mosasauria)

Another group of reptiles to which Marsh made a considerable contribution was the mosasaurs, descendants of carnivorous land reptiles that at some time in the early Cretaceous, and at some place as yet unknown, had invaded the realm of the seas. So common that they were supreme rulers of the Upper Cretaceous seas, they grew to lengths of 30 and exceptionally of 40 feet, with skulls as long as 5 feet, and with backbones made up of more than 100 vertebrae. "On one occasion," Marsh says, "as I rode through a valley washed out of this old ocean bed, I saw no less than seven different skeletons of these monsters in sight at once." By 1880 he had at Yale bones representing not less than 1400 individual mosasaurs, collected, for the most part, by Williston, who became the leading authority on the group. Marsh himself, in eight brief papers, described 7 new genera (2 lost to himself and 2 to Cope) and 18 new species.

Birds with Teeth

The remains of birds are among the rarest of fossils. Their bones, other than those of leg and wing, being very frail, are soon destroyed by the atmosphere, by being crushed, or in other ways; and the buoyancy of their bodies, when afloat, causes the winds and waves to carry their cadavers shoreward where they are sure to be devoured by carnivorous or scavenging animals.

At the outset of his fruitful career in paleontology, Professor Marsh was well aware of these facts, and he was constantly on the hunt for bird remains. His first single bones he secured from New Jersey, in deposits considered at the time to be of Cretaceous age but recently placed in the Eocene; describing them in 1870 as 3 new genera and 5 new species of aquatic birds, he regarded them all as representatives of families now living.

In December of that same year, in the course of the first of his western explorations with a party of students, Marsh found a bird tibia in the Niobrara (Cretaceous) chalk of western Kansas, and it whetted his appetite for better material. The

next year's party, hunting in the Smoky Hill region of the same state, was primed to look for fossil birds; and on November 29 Marsh wrote Professor Dana from San Francisco that among their season's trophies was the headless skeleton of a great bird, found by himself, and parts of four other individuals, found by his students; "on my return," he adds, "I shall describe this unique fossil under the name *Hesperornis regalis*," a promise which he made good in the *American Journal of Science* for January 1872.

The following summer Mudge was collecting in the Smoky Hill country, and Marsh, aware of this, wrote him on September 2 to inquire about his results. Mudge, well disposed toward Marsh from former acquaintance, "practically presented" him with a box of his fossils, regarding which Marsh wrote him on September 25 that "the hollow bones are part of a bird, and the two jaws belong to a small saurian. The latter is peculiar, and I wish I had some of the vertebrae for comparison with other Kansas species." Under this belief, the specimens were described by Marsh as pertaining to different animals, the avian bones being named *Ichthyornis dispar* in October 1872, and the "saurian" bones receiving the appellation *Colonosaurus mudgei* the following month. Not until further preparation of the specimens had revealed a skull and additional portions of both jaws did it become apparent that all the bones belonged to one animal, and that animal a *bird with teeth*. This remarkable discovery was announced in February 1873, in a preliminary paper in which the author modestly remarks that "The fortunate discovery of these interesting fossils . . . does much to break down the old distinction between Birds and Reptiles, which the *Archaeopteryx* has so materially diminished." Williston later spoke of these fossils as "by far the most important specimens of these early years [of Marsh's career], if not the most important of those succeeding," and Osborn in 1931 remarked that they constituted "the most important single palaeontological discovery" of Marsh's life.

In his more extended description of *Hesperornis regalis* in May 1872, Marsh considered it to be related to the Great Northern Diver *(Gavia immer)*, a reference that was later com-

pletely abandoned, as a result of the discovery, by T. H. Russell of the Yale party, of a nearly perfect skeleton of the same bird, including parts of the head and teeth—"an ample reward for the hardships and danger we incurred." When the great Cretaceous diver had been cleaned of all the adhering chalk, it was seen to be a bird 6 feet long and larger than any other known aquatic form, fossil or living. "The maxillary bones are massive," Marsh wrote, "and have throughout their length a deep inferior groove, which was thickly set with sharp, pointed teeth."

These astonishing discoveries were brought into even greater prominence in 1880, when Marsh produced his first monograph, a *magnum opus* entitled *Odontornithes: A Monograph on the Extinct Toothed Birds of North America*, a publication which was said by Henry Woodward, the English paleontologist, to surpass "any which have already appeared devoted to paleontology." This book, a sumptuous royal quarto with 201 pages of text, 40 woodcuts, and 34 lithographic plates, was published as one of the reports of the United States Geological Exploration of the Fortieth Parallel, under the direction of Clarence King, and also as Memoir 1 of the Peabody Museum of Natural History. The text was a most detailed description, bone by bone, of nearly entire skeletons of five species of toothed birds: *Hesperornis regalis, H. crassipes, Ichthyornis dispar, I. victor* (the two last named have curious biconcave vertebrae), and *Apatornis celer*. These descriptions were based upon about fifty different individuals of *Hesperornis* and seventy-seven of *Ichthyornis*—testimony to the care and patience with which Marsh's collectors combed the Kansas chalk for this rare material. "Never before," said Sir Archibald Geikie in his review of the monograph, "has it been possible, we believe, to reconstruct so perfectly so ancient an organism." The plates, which include a full-size restoration of *Ichthyornis* and one of *Hesperornis* half-size, were marvels of reproduction, "combined [with] an artistic finish which has made each plate a kind of finished picture." In defense of such elaborate plates, Marsh says in another paper (1885) that his aim was

"to do full justice to the ample material . . . and where possible, to make the illustrations tell the main story to anatomists. The text of such a

Memoir may soon lose its interest, and belong to the past, but good figures are of permanent value."

Aside from the above monograph, Marsh published fifteen pamphlets and notes on fossil birds between 1870 and 1880, to which he added but eight short articles during the next twenty years. In these twenty-four publications he described 1 new subclass of fossil birds, 2 new orders, 16 new genera (2 are synonyms: *Colonosaurus=Ichthyornis,* and *Lestornis=Hesperornis),* and 43 new species (3 are synonyms, 1 lost to Cope). Of these species, 1 occurs in the Jurassic *(Laopteryx priscus),* 15 in the Cretaceous (12 in the Niobrara, 1 in the Claggett, 2 in the Lance), and 24 in the Cenozoic.

In *Odontornithes,* Marsh says: "The Struthious characters seen in *Hesperornis*, should probably be regarded as evidence of real affinity, and in this case *Hesperornis* would be essentially a carnivorous, swimming Ostrich." This conclusion "did not meet with general acceptance . . . and before long the Ratite affinities of *Hesperornis* were seldom alluded to in scientific literature." When Williston in 1896 described a specimen of *Hesperornis* with some of the feathers in place, Marsh commented that "these feathers are the typical plumage of an Ostrich," and rejoiced that this find proved "beyond dispute" that the nearest affinities of the Odontornithes were with the Ratitae. However, ornithologists of the present day still see no genetic connections here.

In his Nashville address, Marsh stated:

"It is now generally admitted, by biologists who made a study of the vertebrates, that birds have come down to us through the Dinosaurs. . . . The case amounts almost to a demonstration, if we compare, with Dinosaurs, their contemporaries, the Mesozoic birds. The classes of Birds and Reptiles, as now living, are separated by a gulf so profound that a few years since it was cited by the opponents of evolution as the most important break in the animal series, and one which that doctrine could not bridge over. Since then, as Huxley has clearly shown, this gap has been virtually filled by the discovery of bird-like reptiles and reptilian birds. *Compsognathus* [once thought to be a bird but shown by Gegenbaur, Huxley, and Marsh to be a dinosaur], and *Archaeopteryx* ["the most reptilian of birds"] of the Old World and *Ichthyornis* and *Hesperornis* of the New, are

the stepping stones by which the evolutionist of today leads the doubting brother across the shallow remnant of the gulf once thought impassible."

At present, systematists would say that there is not much else besides feathers to distinguish birds from reptiles.

Marsh in 1880 regarded the Odontornithes, or birds with teeth, as a subclass, which he divided into three orders: (1) Odontocolcae, for the *Hesperornis* type; (2) Odontotormae, for the *Ichthyornis* type (in modern classification these are of the order Carinatae); and (3) Saururae of Haeckel, for *Archaeopteryx*. That these oldest types of true birds

"should differ so widely from each other points unmistakably to a great antiquity for the class . . . but the reptilian characters they possess are convergent toward a more generalized type. No Triassic birds are known. . . . [When they are found], if we may judge from Jurassic Mammals and Reptiles, the next classes above and below Birds, the avian forms of that period would still be birds, although with even stronger reptilian features. For the primal forms of the bird-type, we must evidently look to the Paleozoic; and in the rich land fauna from American Permian we may yet hope to find the remains of both Birds and Mammals."

This hope has not yet been realized.

Lucas (in Zittel, 1902 ed.) holds that the birds are "descended without question from reptiles, their affinities with that class are so intimate that Huxley included them both under the common designation of Sauropsida." He objects, however, and correctly, to this merging of birds with reptiles.

Alexander Wetmore, in his *Systematic Classification for the Birds of the World, Revised and Emended* (1934), agrees with Marsh's views as to the systematic relations between the Mesozoic birds and the more recent ones. He refers the Jurassic reptilian birds to the subclass Archaeornithes (ancestral birds), while placing the Cretaceous toothed birds in the subclass Neornithes (true birds), in the superorder Odontognathae (New World toothed birds), and in the orders Hesperornithiformes (*Hesperornis* and *Hargeria*) and Ichthyornithiformes (*Ichthyornis*). While this places them near the ostriches (superorder Palaeognathae or struthious birds) among recent birds, they are not considered as closely allied to that group.

Doctor Wetmore, after kindly reading the above section, made the following comment:

"Marsh's discovery of the toothed birds in the fossil deposits of Kansas though made at so early a day still ranks as one of the outstanding discoveries in palaeornithology in North America. And to his original studies little that is new has been added, except to refute Marsh's belief that these Cretaceous species were closely allied to the living ostriches. In point of fact the toothed birds seem set apart by themselves from all living forms.

"In view of the very definite specialization of *Hesperornis* and its allies for an aquatic existence, and of *Ichthyornis* for life in the air it seems strange that no cursorial type has yet been discovered in Cretaceous deposits, though birds of this kind are found well developed in the Tertiary."

Mammals in General

Originally, Marsh intended to devote most of his time to the study of fossil mammals, but when he began to get the well preserved skeletons of gigantic dinosaurs, he became more and more overwhelmed by the extraordinary evolution shown in this phylum of reptiles, of which very little was known to paleontologists in the seventies. Accordingly, his projected monographs on the Mesozoic mammals, the horses, and the brontotheres failed to materialize, although the handsome one on the Dinocerata appeared in 1885 (author's edition).

Marsh treats of fossil mammals in eighty-five different publications, and his most productive years for describing them were the seventies. In these papers he describes as new 255 species and 120 genera; 38 of the species and 43 of the genera are known to be synonyms. Their variety is so great and their history is so intricate that of most of them little can be said in this memoir. In the list following, these mammals are grouped by orders, and those marked with an asterisk will be presented in more detail. Figures in parentheses indicate synonyms.

Subclass Allotheria. Mesozoic mammals.
    Order Multituberculata.
    Order Triconodonta.
Subclass Eutheria. Viviparous mammals.
    Infraclass Pantotheria.
        Order Symmetrodonta.

Infraclass Didelphia. Non-placental mammals.
  Order Marsupialia, opossums, kangaroos, etc.
  Of the above orders, Marsh described:
    * Late Jurassic mammals, 15 genera (9) and 25 species (6).
    * Late Cretaceous mammals, 19 genera (10) and 37 species (21).

Infraclass Monodelphia. Placental mammals. All of Cenozoic time.
  Has the following orders:
    Insectivora, 12 genera (6) and 18 species (3).
    Tillodontia Marsh, 1 genus and 4 species.
    Taeniodonta, of uncertain systematic relationship, 2 genera and 2 species.
    Cheiroptera. None of the "bats" described by Marsh belong in this order.
    Xenarthra, ground sloths or Gravigrada, 1 genus and 2 species.
    Rodentia, 5 genera (2) and 11 species (1).
    Carnivora, 11 genera (3) and 17 species (1).
    Primates, 5 genera (3) and 10 species (1). Five species are lemurs and five are tarsioids, the first of these primitive primates to be found in America.
    Condylarthra, primitive ungulates, 2 genera (1) and 3 species (1).
    * Amblypoda, short-footed ungulates, 3 genera and 23 species of * dinoceres.
    Coryphodontia, 1 species.
    Sirenia, sea cows, 1 genus and 1 species.
    Perissodactyla, hoofed animals with an odd number of toes, including the * horses with 8 genera (2) and 19 species (1); the * brontotheres with 13 genera (4) and 21 species (5); the tapirs with 2 genera and 3 species; and the rhinoceroses with 5 genera and 14 species.
    Ancylopoda, clawed animals with an odd number of toes, 1 genus and 3 species.
    Artiodactyla, cloven-hoofed animals with an even number of toes, including oreodonts, swine-like mammals, camels, deer, antelopes, etc., 21 genera (4) and 49 species (1).

Mesozoic Mammals

From the phylogenetic viewpoint, the *Jurassic mammals* are probably the most significant of the whole class Mammalia, but unfortunately these fossils are amongst the greatest rarities, nearly all the known specimens being fragmentary jaws and isolated teeth. Even so, as G. G. Simpson said in 1928, "one is at least dealing for the most part with jaws with their included teeth and direct comparisons between the established genera are possible in most cases".

The first Mesozoic mammal jaw was found in England in 1764, but it was not recognized as such until 1824, when the great anatomist, Cuvier, saw it and pronounced it mammalian. In 1871 it was named *Amphilestes broderipii* by Owen. Professor Marsh, from his European studies, was fully aware of the desirability of obtaining more of these early mammals, and he was constantly urging his field men to be on the watch for small fossils. Due to this prodding, one of his best collectors, W. H. Reed, excavating for dinosaurs at Como Bluff, Wyoming, in 1878, found a good lower jaw of a mammal about the size of a weasel. Marsh named this find *Dryolestes priscus*, and added that it represented a marsupial "allied to the existing opossums". Late in 1879, Marsh named more of these "medieval" mammals, saying that they show "such a resemblance to known types from the Purbeck of England, that some connection between the two faunae is clearly implied." Further study convinced him that these mammals

"cannot be satisfactorily placed in any of the present orders. This appears to be equally true of the European forms. . . . With the exception of a very few aberrant forms, the known Mesozoic mammals may be placed in a single order, which may appropriately be named Pantotheria."

This order is still recognized by some systematists, and Marsh's order Allotheria is now raised to a subclass.

These finds led to a more systematic search for mammals, notably in the famous Quarry 9 at Como, from which, thanks to a year's careful and persistent search, nearly all the more than 400-500 separate specimens of American Jurassic mammals have come. The cost involved in getting these tiny fossils makes them worth more than their weight in gold, and they are among the great treasures of the museums at Yale and at Washington.

Carrying out Marsh's original intention, all the Mesozoic mammal material thus collected has now been elaborately monographed by George G. Simpson (1928), who finds 44 Jurassic species in 23 genera. However, even our present knowledge of these early mammals, garnered over more than a century of endeavor by many paleontologists, represents, according to Simpson, only "lights in the vast darkness of the Age of Reptiles—and very dim lights most of them are".

No authentic *Cretaceous mammals* appear to have been found anywhere until 1882, when isolated teeth of such were discovered by J. L. Wortman in the Laramie formation of Wyoming. It was, however, not until 1889 that they began to be found in any quantity, Hatcher writing Marsh on May 20 that he was sending to New Haven by registered mail "a package containing some 4 or 5 species of Laramie mammals. . . I hope you will be pleased and will not despise them because they are few in number. They are by no means abundant, the few I send you requiring several days careful search." Marsh was pleased indeed, so much so that on June 8 he telegraphed Hatcher to stop work on ceratopsian skeletons and go after mammals entirely, for which he then had four different localities. Within the next four years, Hatcher, assisted by C. E. Beecher and other collectors, sent to Yale about 5000 teeth and some jaws and skeletal parts. As early as July 1889 Marsh had a paper in print describing 12 genera and 18 species of these Cretaceous mammals, and announcing that he had in preparation for the United States Geological Survey a memoir on this "rich mammalian fauna". In this paper he gives credit to Hatcher for the discovery of "material for a new chapter in paleontology".

Simpson's study finds that these Cretaceous mammals are still very inadequately known, because of their usual isolated occurrence as teeth "which cannot be associated into natural genera in the majority of cases . . . the characters of two consecutive teeth of a single genus cannot be determined in many instances." Marsh was aware of the necessarily artificial nature of his classification of these Cretaceous mammals; as Simpson says, Marsh had to resort to an analytical basis, giving names "not to distinct animals but to different types of teeth. Under the circumstances there was much to be said for this procedure." Even yet the time is not at hand for a synthesis of these teeth into genera and species based on entire animals, and Simpson concludes that "a revision, in the strictest sense, is impossible".

The Curious Dinocerata

The relatively abundant short-footed amblypods for which Marsh erected the suborder Dinocerata originated in Wyoming

during the late Paleocene and died out at the close of the Eocene epoch, being the most striking and characteristic animals of middle Eocene (Bridger) time. They are also known in Mongolia, whither they migrated from America. Some of the larger forms, standing 6-7 feet high at the shoulders, were elephant-like in bodily build, but they had no trunks and their curious heads were wholly unlike those of the proboscideans in that they bore three pairs of horns; and, in the males at least, the upper canine teeth were drawn out into long recurved saber-like tusks that must have been terrible weapons, although the manner of their use is unknown. The brain was exceedingly small.

While Leidy was the first to discover bones of the Dinocerata, his material was very fragmentary. To Marsh belongs, as Wortman said in 1899, "the credit of the final determination of their structure and affinities; he classified them in a separate and distinct order, Dinocerata, a name which has been very widely adopted by naturalists."

In September 1870, Professor Marsh, with a large party of Yale students, explored the Green River basin of western Wyoming under military escort. Here they found a large "boneyard", in which mammal remains were the most abundant fossils. Among these was a partial skeleton which Marsh the following year referred doubtfully to Leidy's genus *Titanotherium,* calling the species *T. ? anceps.* He returned a number of times to this basin, and to other nearly as rich ones in the Green River country, and brought back many more specimens of this group, which he named Dinocerata in 1873, and of which he finally had more than two hundred individuals, including some twenty skulls in good condition—striking testimony to the tenacity and thoroughness so characteristic of Marsh as a collector.

Between 1872 and 1885, Marsh issued no fewer than thirty-four papers treating of the Dinocerata, the series culminating in the quarto volume entitled *Dinocerata, a Monograph of an Extinct Order of Gigantic Mammals,* which, illustrated by 200 woodcuts and 56 lithographic plates, ranks among his best studies. This volume describes 3 of Marsh's genera, *Dinoceras, Tinoceras,* and *Laoceras* (subgenus), and 1 of Leidy's, *Uinta-*

therium, the four together having 23 species named by Marsh and 6 named by other authors (Cope 3, Osborn 2, Leidy 1).

It was the naming of these mammals that led in large part to the intense rivalry between Cope and Marsh, into which the older Leidy—a man "insensible to and unaffected by the ordinary passions of ambition or rivalry"—was unfortunately drawn.

While a student in Germany, Marsh had met Cope, and previous to 1870 they had exchanged friendly letters. Shortly after Marsh's return, he called on Cope at Haddonfield, New Jersey, collected fossils with him, and purchased some of his material. Marsh's expedition to the West in 1870, however, and its great success in securing vertebrate fossils, as reflected in his papers of 1871 describing 4 new genera and 27 new species, showed the Philadelphia group, represented by Leidy, Cope, and Hayden, that here was a very real competitor.

Hayden had begun exploring on his own account in the Missouri River country as early as 1854; in 1856 he joined Lieut. G. K. Warren's survey, and in 1859 that of Capt. W. F. Raynolds. In 1867, he was called on to organize the United States Geological Survey of the Territories, which, at the time of its formation, was one of four separate national surveys in the Rocky Mountain country. All the vertebrate fossils collected by Hayden had been turned over for study to Leidy and later to Cope, and up to and including 1870 the former had described from the West slightly more than 100 new species. After the early seventies, Leidy dropped more and more out of the field of western vertebrate paleontology, but Cope in 1872 published thirty-four papers and notes, a strong indication of his intention to remain there.

When Congress abolished the four independent surveys in 1879, and called for a single new organization, the United States Geological Survey, to be headed by Clarence King—a reorganization with which Marsh had considerable to do—Cope and Hayden found themselves supplanted in their work for the federal government.

Returning to the battleground of the Dinocerata, Marsh published thirty-five papers on this group between 1871 and 1884, and Cope at least twenty-nine. Seven of Cope's papers

describing new genera and species were dated 1872; Marsh doubted this date and he set to work to find out the actual dates of issue, presenting his findings in ten different papers appearing during 1873. As a result of this tangle of conflicting dates, the taxonomy of the Dinocerata is even yet not settled, nor will it be until some judicially minded vertebrate paleontologist, fully conversant with the International Rules of Nomenclature, studies all the great mass of material in the various museums.

The Great Brontotheres

On the third Yale expedition to the West, in 1872, two members of the party, H. B. Sargent and J. W. Griswold, found remains of a huge new mammal, to which Marsh in the following year gave the interesting name *Brontotherium gigas*, the "great thunder beast." Marsh showed this striking new type to be a true perissodactyl, and, according to Osborn, "was able in a very few words to throw a flood of light upon the characters of the skeleton." These animals are often called titanotheres, but since the generic name *Titanotherium* (of Leidy) no longer stands, it would seem that for the name of the group we should fall back upon Marsh's term brontotheres; this is also W. B. Scott's conclusion (1937). These creatures once roamed in great herds over what is now the Great Plains of eastern Colorado, Wyoming, Dakota, and Nebraska. Their brains were no larger than a man's fist even in the largest of the group, which attained almost the bulk of an elephant. As Scott says, these great beasts "were even more dull and stupid than are modern rhinoceroses."

Although elephantine in bulk, the brontotheres were less heavily built proportionally, and stood somewhat higher. The head was saddle-shaped, with a blunt horn on either side of the nose. The first of the group were comparatively low of stature, and their "horns" were small knobs, well back of the eyes. With the passage of geologic time, the horns grew longer and the animals larger, so that the skull became a yard long and the horns a foot high and on the very end of the nose. These animals arose early in the Eocene of North America, and died out in the Oligocene, a geologic interval of about ten million years. Recently they have been discovered in the Eocene and

Oligocene of Mongolia. Their genetic evolution took place along four main stems, but counting all the side branches, Osborn later indicated not less than eight phyletic lines, with 23 genera, 8 of which were named by Marsh.

Between 1873 and 1891, Marsh published thirteen papers on these brontotheres. In 1889, he presented a restoration of *Brontops robustus,* pointing out that it represented the largest animal of its time. Bones of this animal were first found in 1874 by H. C. Clifford, south of the Black Hills near Chadron, Nebraska, not far from the White River, but not until 1886 were most of its parts finally recovered, and the left hind leg is still missing; the mounted skeleton is one of the greatest treasures in the Peabody Museum.

In the early 1880's Marsh planned a large and well illustrated monograph on the brontotheres, and for it made sixty lithographic plates, but at the time of his death in 1899 he had not even begun to prepare the manuscript. The United States Geological Survey in 1900 transferred the task of writing this monograph to H. F. Osborn, but he was not able to finish it until 1919, and another ten years passed before the two handsome volumes appeared under the title *The Titanotheres of Ancient Wyoming, Dakota, and Nebraska.* They form the most far-reaching work on a single group of vertebrate fossils ever published, and it is pleasant to read in them that Marsh

"made the largest and most valuable contributions to our knowledge of this family and of its evolution. He planned the monumental field work of John Bell Hatcher, by which the great collection for the United States National Museum was made [which has more than 158 skulls and jaws] and he supervised the preparation of the sixty lithographic plates, which are here reproduced."

Increase of Intelligence in Geologic Time

From 1870 on, whenever Marsh had sufficient material, it was his rule to have his preparators section the rear end of fossil skulls and clean out all the rock or crystalline material in the brain cavity. Into this cavity was then poured warm gelatine, which, because of its marked pliability, could be easily pulled out of the cavity, when cold, without tearing off any of the projecting parts. From this gelatine cast a more permanent

mold would be made that permitted the taking of other replicas.

By 1874, he had brain casts of many Cenozoic mammals, and these enabled him to make a first attempt at a generalization regarding brain growth in geologic time. On the evening of June 17, he presented his conclusions before the Connecticut Academy of Arts and Sciences in New Haven. The Eocene mammals, he said,

"all appear to have had small brains, and in some of them the brain cavity was hardly more capacious than in the higher reptiles. The largest Eocene mammals are the *Dinocerata*, which were but little inferior to the elephant in bulk. In *Dinoceras* . . . the brain cavity is not more than one-eighth the average size of that in existing Rhinoceroses. . . . The gigantic mammals of the American Miocene [=Oligocene] are the *Brontotheridae*, which equalled the *Dinocerata* in size. In *Brontotherium* Marsh . . . the brain cavity is . . . about the size of the brain in the Indian *Rhinoceros*. In the Pliocene strata of the West, a species of *Mastodon* is the largest mammal, and although but little superior in absolute size to *Brontotherium*, it had a very much larger brain, but not equal to that of existing Proboscidians. The Tapiroid ungulates of the Eocene had small brain cavities, much smaller than their allies, the Miocene *Rhinocerotidae*. The Pliocene representatives of the latter group had well developed brains, but proportionally smaller than living species. A similar progression in brain capacity seems to be well marked in the equine mammals."

In 1876, Marsh briefly recapitulated his knowledge as follows:

"*First*, all Tertiary mammals had small brains; *second*, there was a gradual increase in the size of the brain during this period; *third*, this increase was mainly confined to the cerebral hemispheres, or higher portion of the brain; *fourth*, in some groups, the convolutions of the brain have gradually become more complicated; *fifth*, in some, the cerebellum and olfactory lobes have been diminished in size."

These statements he repeated in his Nashville address the following year, adding:

"In the long struggle for existence during Tertiary time, the big brains won, then as now; and the increasing power thus gained rendered useless many structures inherited from primitive ancestors, but no longer adapted to new conditions."

In his Presidential Address at Saratoga in 1879, he went still further, saying:

"More recent researches render it probable that the same general law of brain-growth holds good for birds and reptiles from the Mesozoic to the

present time. The Cretaceous birds, that have been investigated with reference to this point, had brains only about one-third as large in proportion as those nearest allied among living species. The Dinosaurs from our Western Jurassic follow the same law, and had brain cavities vastly smaller than any existing reptiles."

To the five conclusions regarding brain growth stated in 1876, and later presented as an "outline of a general law of brain growth," Marsh made two additions in the Dinocerata monograph (1885), as follows:

6. "The brain of a mammal belonging to a vigorous race, fitted for a long survival, is larger than the average brain of that period in the same group."

7. "The brain of a mammal of a declining race is smaller than the average of its contemporaries of the same group."

HONORS

When one considers that in Professor Marsh's time, honors and other evidences of distinguished achievement were not as numerous as they are now, those that he received make an impressive list.

On the academic side, his record includes the class valedictory at Andover, a High Orations stand at graduation from Yale, a Berkeley Scholarship, and election to Phi Beta Kappa; he received an honorary Ph.D. from Heidelberg University at its 500th anniversary in 1886, and Harvard's doctorate of laws in the same year, at its 250th anniversary.

President of the American Association for the Advancement of Science in 1878, he was vice-president of the National Academy of Sciences from 1878 to 1883, and its president from 1883 to 1895. He was vertebrate paleontologist of the United States Geological Survey from 1882 to 1899, and honorary curator of vertebrate paleontology in the United States National Museum from 1887 until 1899.

From the Geological Society of London, of which he was elected a Fellow in 1863 and a Foreign Member in 1898, he received the Bigsby Medal in 1877; and twenty years later he received Vertebrate Paleontology's *cordon bleu*, the Cuvier Prize from the Institute de France, becoming Correspondent of the French Academy the next year.

He was a member or an honorary member of forty-one scientific societies or academies, and six of a non-scientific nature, distributed as follows: United States, 26; England, 4; Belgium, 4; Germany, 4; France, 2; and one each in Canada, Mexico, Argentina, Ireland, Denmark, Italy, and Russia.

His epitaph, written by his lifelong mentor, Professor Brush, reads: "To Yale he gave his services, his collections and his estate."

"Here are they to whom, from the depths of space, were whispered in the night watches its long hidden secrets. There, too, are those who, in the silence of the laboratory, rejoiced in the fertile marriage of the elements, or they who, like confessors, heard from dead bones or rock or flower the immeasurable history of the silent ages of earth."

S. WEIR MITCHELL,
At the Jubilee banquet of the National Academy of Sciences.

BIBLIOGRAPHY

OF

Othniel Charles Marsh

1831–1899

Compiled from three sources: a list of Professor Marsh's papers, prepared by Miss Lucy Peck Bush, his secretary for many years, and published privately by Professor Marsh from time to time; the bibliography appearing in Professor Charles E. Beecher's memorial; and one reprinted from "Bibliographies of the Officers of Yale University, covering the years 1861–1892." About three hundred titles are listed here.

1861

The Gold of Nova Scotia. AMER. JOUR. SCI. (2), vol. 32, pp. 395–400. Abstract: CANADIAN NAT., vol. 6, pp. 427–430.

1862

On the Saurian Vertebrae from Nova Scotia. AMER. JOUR. SCI. (2), vol. 33, p. 278.

Description of the Remains of a new Enaliosaurian (*Eosaurus acadianus*), from the Coal Formation of Nova Scotia. *Ibid.*, vol. 34, pp. 1–16, pls. i–ii. Abstract: CANADIAN NAT., vol. 7, pp. 205–213; QUART. JOUR. GEOL. SOC. LONDON, vol. 19, pp. 52–56, 1863.

1863

Catalogue of Mineral Localities in New Brunswick, Nova Scotia, and Newfoundland. AMER. JOUR. SCI. (2), vol. 35, pp. 210–218. Abstract: In J. D. Dana, "Treatise on Mineralogy," 5th ed., pp. 789–791, 1868.

On the Science of the International Exhibition. AMER. JOUR. SCI. (2), vol. 35, pp. 256–259.

1864

Notice of a new Fossil Annelid (*Helminthodes antiquus*), from the Lithographic Slates of Solenhofen. *Ibid.*, vol. 38, p. 415. Abstract: ZEITS. DEUTSCH. GEOL. GESELL., vol. 16, p. 363; N. JAHRB., p. 57, 1865.

1865

[Ueber einen Pterodactylus von Eichstädt, alpinen Muschelkalk und *Solanocrinus costatus*.] ZEITS. DEUTSCH. GEOL. GESELL., vol. 17, p. 13.

[New Genus of Jurassic Annelides (*Ischyracanthus*).] *Ibid.*, vol. 17, p. 267.

[Double Lobe-lines of *Ceratites nodosus*.] *Ibid.*, vol. 17, pp. 267–269.

1866

Description of an ancient Sepulchral Mound near Newark, Ohio. Amer. Jour. Sci. (2), vol. 42, pp. 1–11.

1867

Discovery of additional *Mastodon* Remains at Cohoes, New York. *Ibid.*, vol. 43, pp. 115–116.

A Catalogue of Official Reports upon Geological Surveys of the United States and British Provinces. *Ibid.*, vol. 43, pp. 116–121, 399–404. Reprint, pp. 1–14 (slightly different).

Notice of a new Genus of Fossil Sponges from the Lower Silurian. *Ibid.*, vol. 44, p. 88. Abstract: Canadian Nat., n.s., vol. 3, p. 301, 1868.

Contributions to the Mineralogy of Nova Scotia. No. 1. Ledererite identical with Gmelinite. Amer. Jour. Sci. (2), vol. 44, pp. 362–367.

1868

On the *Palaeotrochis* of Emmons from North Carolina. *Ibid.*, vol. 45, pp. 217–219.

On the Origin of the so-called *Lignilites*, or *Epsomites*. Abstract: Proc. Amer. Assoc. Adv. Sci., Burlington meeting, 1867, vol. 16, pp. 135–143; Canadian Nat., n.s., vol. 3, p. 293.

On certain Effects produced upon Fossils by Weathering. Abstract: Canadian Nat., n.s., vol. 3, p. 305.

Observations on the Metamorphosis of *Siredon* into *Amblystoma*. Amer. Jour. Sci. (2), vol. 46, pp. 364–374, 1 pl.; Zoologist, London, vol. 4, pp. 1569–1580, 1869. Abstract: Amer. Nat., vol. 2, p. 493; Proc. Boston Soc. Nat. Hist., vol. 12, pp. 97-98, 1869.

Notice of a new and diminutive Species of Fossil Horse (*Equus parvulus*), from the Tertiary of Nebraska. Amer. Jour. Sci. (2), vol. 46, pp. 374-375; Ann. Mag. Nat. Hist. (4), vol. 3, pp. 95–96, 1869. Abstract: N. Jahrb., p. 767, 1869.

1869

Notice of some new Reptilian Remains from the Cretaceous of Brazil. Amer. Jour. Sci. (2), vol. 47, pp. 390–392. Ann. Mag. Nat. Hist. (4), vol. 3, pp. 442–444. Abstract: N. Jahrb., p. 112, 1871.

Description of a new and gigantic Fossil Serpent (*Dinophis grandis*), from the Tertiary of New Jersey. Amer. Jour. Sci. (2), vol. 48, pp. 397–400. Abstract: Amer. Nat., vol. 4, p. 254, 1870.

Description of a new Species of *Protichnites* from the Potsdam Sandstone of New York. Proc. Amer. Assoc. Adv. Sci., 1868, vol. 17, pp. 322–324; Amer. Jour. Sci. (2), vol. 48, pp. 46–49, 1 pl. Abstract: N. Jahrb., p. 536, 1871.

On the Preservation of Color in Fossils from Palaeozoic Formations. Proc. Amer. Assoc. Adv. Sci., 1868, vol. 17, pp. 325–326.

On a remarkable Locality of Vertebrate Remains in the Tertiary of Nebraska. Abstract: Canadian Nat., vol. 4, pp. 322–323.

[The Cardiff Giant.] New York Herald, Dec. 1, 1869, p. 8. Reprinted from Buffalo Courier, Nov. 29.

Notice of some new Mosasauroid Reptiles from the Greensand of New Jersey. Amer. Jour. Sci. (2), vol. 48, pp. 392–397. Abstract: Canadian Nat., vol. 4, p. 331; Amer. Nat., vol. 4, p. 62, 1870; Geol. Mag., vol. 7, pp. 376-377, 1870; N. Jahrb., p. 526, 1870.

1870

Notice of some new Tertiary and Cretaceous Fishes. Proc. Amer. Assoc. Adv. Sci., 1869, vol. 18, pp. 227–230.

Notice of some Fossil Birds from the Cretaceous and Tertiary Formations of the United States. Amer. Jour. Sci. (2), vol. 49, pp. 205–217. Abstract: Proc. Acad. Nat. Sci. Philadelphia, vol. 22, pp. 5–6; Geol. Mag., vol. 7, pp. 377–378; N. Jahrb., p. 512; Zeits. Gesammt. Naturw., vol. 5, pp. 263–264, 1872.

Note on the Remains of Fossil Birds. Amer. Jour. Sci. (2), vol. 49, p. 272.

Notice of a new Species of Gavial from the Eocene of New Jersey. *Ibid.,* vol. 50, pp. 97–99; Geol. Mag., vol. 7, p. 427.

[New Vertebrate Fossils from New Jersey.] Proc. Acad. Nat. Sci. Philadelphia, vol. 22, pp. 2–3.

[New Fossil Turkey.] *Ibid.,* p. 11; (with additions) Amer. Jour. Sci. (3), vol. 4, pp. 260–261, 1872. Abstract: Amer. Nat., vol. 4, p. 317, 1871.

[New Fossil Peccary from New Jersey.] Proc. Acad. Nat. Sci. Philadelphia, vol. 22, p. 11.

Rocky Mountain Expedition.—Discovery of the *Mauvaises Terres* Formation in Colorado. Amer. Jour. Sci. (2), vol. 50, p. 292; Canadian Nat., vol. 5, p. 240, 1871.

1871

Note on *Lophiodon* from the Miocene of New Jersey. Proc. Acad. Nat. Sci. Philadelphia, vol. 23, pp. 9–10.

New Reptiles and Fishes from the Cretaceous and Tertiary Formations. *Ibid.,* pp. 103–105.

Scientific Expedition to the Rocky Mountains. Amer. Jour. Sci. (3), vol. 1, pp. 142–143.

On the Geology of the eastern Uintah Mountains. *Ibid.,* vol. 1, pp. 191-198.

Notice of a Fossil Forest in the Tertiary of California. *Ibid.,* vol. 1, pp. 266–268. Abstract: N. Jahrb., pp. 892–893.

Description of some new Fossil Serpents from the Tertiary Deposits of Wyoming. AMER. JOUR. SCI. (3), vol. 1, pp. 322–329; BOLL. R. COMITATO GEOL. D'ITALIA, vol. 3, pp. 273–278, 1872. Abstract: N. JAHRB., p. 986.

Notice of some new Fossil Reptiles from the Cretaceous and Tertiary Formations. AMER. JOUR. SCI. (3), vol. 1, pp. 447–459; BOLL. R. COMITATO GEOL. D'ITALIA, vol. 3, pp. 278–283, 338–343, 1872. Abstract: N. JAHRB., p. 890; POP. SCI. REV., London, p. 436.

Note on a new and gigantic Species of Pterodactyle. AMER. JOUR. SCI. (3), vol. 1, p. 472. Abstract: N. JAHRB., p. 890.

Notice of some new Fossil Mammals from the Tertiary Formation. AMER. JOUR. SCI. (3), vol. 2, pp. 35–44; BOLL. R. COMITATO GEOL. D'ITALIA, vol. 3, pp. 343–350, 1872.

Notice of some new Fossil Mammals and Birds from the Tertiary Formation of the West. AMER. JOUR. SCI. (3), vol. 2, pp. 120–127; BOLL. R. COMITATO GEOL. D'ITALIA, vol. 3, pp. 350–353, 1872. Abstract: N. JAHRB., pp. 106–107, 1872; ZEITS. GESAMMT. NATURW., vol. 5, p. 264, 1872.

1872

Discovery of a remarkable Fossil Bird. AMER. JOUR. SCI. (3), vol. 3, pp. 56–57. Abstract: NATURE, vol. 5, p. 348, Feb. 29, 1872.

Remarks on Explorations in the Rocky Mountains, Oregon, etc. Abstract: PROC. CALIFORNIA ACAD. NAT. SCI., vol. 4, p. 200.

Discovery of additional Remains of *Pterosauria*, with Descriptions of two new Species. AMER. JOUR. SCI. (3), vol. 3, pp. 241–248. Abstract: N. JAHRB., p. 983; NATURE, vol. 5, p. 151, June 20, 1872.

Discovery of the dermal Scutes of Mosasauroid Reptiles. AMER. JOUR. SCI. (3), vol. 3, pp. 290–292.

Notice of a new Species of *Hadrosaurus*. *Ibid.*, vol. 3, p. 301.

Preliminary Description of *Hesperornis regalis*, with Notices of four other new Species of Cretaceous Birds. *Ibid.*, vol. 3, pp. 360–365; ANN. MAG. NAT. HIST. (4), vol. 10, pp. 212–217; BOLL. R. COMITATO GEOL. D'ITALIA, vol. 3, pp. 211–217. Abstract: NATURE, vol. 6, pp. 90, 94, May 30, 1872; ZEITS. GESAMMT. NATURW., vol. 5, p. 263; POP. SCI. REV., London, pp. 316–317; N. JAHRB., p. 983.

On the Structure of the Skull and Limbs in Mosasauroid Reptiles, with Descriptions of new Genera and Species. AMER. JOUR. SCI. (3), vol. 3, pp. 448-464, pls. x-xiii. Abstract: N. JAHRB., p. 983; JOUR. ZOOL., Paris, vol. 2, pp. 533-536, 1873.

Note "On Two new Ornithosaurians from Kansas." AMER. JOUR. SCI. (3), vol. 3, pp. 374–375.

Bowlders in Coal. AMER. NAT., vol. 6, p. 439.

Preliminary Description of new Tertiary Mammals. Pt. I. AMER. JOUR. SCI. (3), vol. 4, pp. 122–128. Abstract: N. JAHRB., pp. 990–991.

Yale College Expedition to the Rocky Mountains and Pacific Coast. Amer. Jour. Sci. (3), vol. 3, p. 146.

Note on *Rhinosaurus*. *Ibid.,* vol. 3, p. 147.

Preliminary Description of new Tertiary Mammals. Pts. II, III, and IV. *Ibid.,* vol. 3, pp. 202–224. Abstract: N. Jahrb., pp. 990–991.

Notice of some new Tertiary and Post-Tertiary Birds. Amer. Jour. Sci. (3), vol. 3, pp. 256–262. Abstract: N. Jahrb., p. 220, 1873.

Preliminary Description of new Tertiary Reptiles. Pts. I and II. Amer. Jour. Sci. (3), vol. 4, pp. 298–309. Abstract: N. Jahrb., p. 984.

Note on *Tinoceras anceps*. Amer. Jour. Sci. (3), vol. 4, p. 322. Abstract: Pop. Sci. Rev., London, p. 94, 1873.

Notice of a new Species of *Tinoceras*. Amer. Jour. Sci. (3), vol. 4, p. 323.

Notice of some remarkable Fossil Mammals. *Ibid.,* vol. 4, pp. 343–344.

Notice of a new and remarkable Fossil Bird. *Ibid.,* vol. 4, p. 344; Ann. Mag. Nat. Hist. (4), vol. 11, p. 80, 1873; Amer. Nat., vol. 7, p. 50, 1873. Abstract: Pop. Sci. Rev., London, p. 97, 1873; Zeits. Gesammt. Naturw., vol. 6, p. 532.

Discovery of Fossil *Quadrumana* in the Eocene of Wyoming. Amer. Jour. Sci. (3), vol. 4, pp. 405–406; Geol. Mag., vol. 10, p. 33, 1873; Amer. Nat., vol. 7, pp. 179–180, 1873.

Note on a new Genus of Carnivores from the Tertiary of Wyoming. Amer. Jour. Sci. (3), vol. 4, p. 406; Geol. Mag., vol. 10, pp. 33–34, 1873.

Notice of a new Reptile from the Cretaceous. Amer. Jour. Sci. (3), vol. 4, p. 406.

Synopsis of American Fossil Birds. In Coues's "Key to North American Birds," pp. 347–350, Salem.

1873

Return of the Yale College Expedition. Amer. Jour. Sci. (3), vol. 5, pp. 71–72.

Notice of a new Species of *Ichthyornis*. *Ibid.,* vol. 5, p. 74. Abstract: Zeits. Gesammt. Naturw., vol. 6, p. 532, 1872; Nature, vol. 7, p. 310, Feb. 20, 1873; Jour. Zool., Paris, vol. 2, pp. 40–41.

On some of Professor Cope's recent Investigations. Amer. Nat., vol. 7, pp. 51–52.

On the gigantic Fossil Mammals of the Order *Dinocerata*. Amer. Jour. Sci. (3), vol. 5, pp. 117–122, pls. i–ii; Ann. Sci. Nat. (Zool.), Paris, vol. 17, pp. 1–8, pl. 21; Jour. Zool., Paris, vol. 2, pp. 160–168. Abstract: Zeits. Gesammt. Naturw., vol. 6, pp. 533–534, 1872; Geol. Mag., vol. 10, pp. 115–116; Nature, vol. 7, p. 366, March 13, 1873; Pop. Sci. Rev., London, p. 213; N. Jahrb., pp. 334–335, 1875.

On a new Sub-class of Fossil Birds (*Odontornithes*). Amer. Jour. Sci. (3), vol. 5, pp. 161–162; Amer. Nat., vol. 7, pp. 115–117; Ann. Mag. Nat. Hist. (4), vol. 11, pp. 233–234; Ann. Sci. Nat. (Zool.), Paris,

vol. 17, pp. 8–10. Abstract: Pop. Sci. Rev., London, pp. 210–211; N. Jahrb., p. 334.

Discovery of new Rocky Mountain fossils. Proc. Amer. Philos. Soc., vol. 12, pp. 578–579; Ann. Sci. Géol., Paris, vol. 3, pp. 99–100.

Fossil Birds from the Cretaceous of North America. Amer. Jour. Sci. (3), vol. 5, pp. 229–230.

Notes on the Dates of some of Prof. Cope's recent Papers. *Ibid.,* vol. 5, pp. 235–236; Amer. Nat., vol. 7, p. 173.

The Fossil Mammals of the Order *Dinocerata.* Amer. Nat., vol. 7, pp. 146–153, pls. i–ii.

Additional Observations on the *Dinocerata.* Amer. Jour. Sci. (3), vol. 5, pp. 293–296. Abstract: Nature, vol. 7, p. 491, April 24, 1873.

Supplementary Note on the *Dinocerata.* Amer. Jour. Sci. (3), vol. 5, pp. 310–311.

On the Genus *Tinoceras* and its Allies. Amer. Nat., vol. 7, pp. 217–218.

Notice of new Tertiary Mammals. Amer. Jour. Sci. (3), vol. 5, pp. 407–410. Abstract: Nature, vol. 8, p. 76, May 22, 1873.

On the Dates of Prof. Cope's recent Publications. Amer. Nat., vol. 7, pp. 303–306.

Tinoceras and its Allies. *Ibid.,* vol. 7, pp. 306–308.

Reply to Professor Cope's Explanation. *Ibid.*, vol. 7, Appendix, pp. i–ix.

Notice of new Tertiary Mammals (continued). Amer. Jour. Sci. (3), vol. 5, pp. 485–488.

New Observations on the *Dinocerata. Ibid.*, vol. 6, pp. 300–301.

On the gigantic Mammals of the American Eocene. Proc. Amer. Philos. Soc., vol. 13, pp. 255–256.

1874

Return of Rocky Mountain Expedition. Amer. Jour. Sci. (3), vol. 7, pp. 62–63. Abstract: Nature, vol. 9, p. 273, Feb. 5, 1874.

On the Structure and Affinities of the *Brontotheridae.* Amer. Jour. Sci. (3), vol. 7, pp. 81–86, pls. i–ii. Amer. Nat., vol. 8, pp. 79–85. Abstract: Nature, vol. 9, p. 227, Jan. 22, 1874; Jour. Zool., Paris, vol. 3, pp. 61–62.

Notice of new equine Mammals from the Tertiary Formation. Amer. Jour. Sci. (3), vol. 7, pp. 247–258, 5 figs.; Ann. Mag. Nat. Hist. (4), vol. 13, pp. 397–400. Abstract: Nature, vol. 9, p. 390, March 19, 1874; Zeits. Gesammt. Naturw., vol. 10, pp. 463–464; Jour. Zool., Paris, vol. 3, pp. 90–92.

Fossil Horses in America. Amer. Nat., vol. 8, pp. 288–294, figs. 73–75. Abstract: N. Jahrb., p. 774, 1875.

Notice of new Tertiary Mammals. Pt. III. Amer. Jour. Sci. (3), vol. 7, pp. 531–534. Abstract: N. Jahrb., p. 774, 1875.

Small size of the Brain in Tertiary Mammals. Amer. Jour. Sci. (3), vol. 8, pp. 66–67; Amer. Nat., vol. 8, pp. 503–504; Ann. Mag. Nat. Hist. (4), vol. 14, p. 167; Jour. Zool., Paris, vol. 3, pp. 326–327. Abstract: Nature, vol. 10, p. 273, Aug. 6, 1874; N. Jahrb., p. 772.

1875

Ancient Lake Basins of the Rocky Mountain Region. Pt. I. Amer. Jour. Sci. (3), vol. 9, pp. 49–52. Abstract: Amer. Nat., vol. 9, p. 119; Geol. Mag. (2), vol. 2, pp. 232–233.

Return of Prof. Marsh's Expedition. Amer. Jour. Sci. (3), vol. 9, p. 62.

New Order of Eocene Mammals. *Ibid.*, vol. 9, p. 221; Amer. Nat., vol. 9, pp. 182–183; Nature, vol. 11, p. 368, March 11, 1875; Ann. Mag. Nat. Hist. (4), vol. 15, p. 307; Jour. Zool., Paris, vol. 4, pp. 70–71; N. Jahrb., p. 332, 1876; Pop. Sci. Rev., London, p. 207.

Notice of new Tertiary Mammals. Pt. IV. Amer. Jour. Sci. (3), vol. 9, pp. 239–250. Abstract: Jour. Zool., Paris, vol. 4, pp. 98–99; N. Jahrb., pp. 332–333, 1876.

A Statement of Affairs at Red Cloud Agency, made to the President of the United States. Rept. Special Commission to investigate Affairs Red Cloud Indian Agency, pp. 1–113, Washington. Author's ed., pp. 1–38, New Haven.

Note on Reindeer Bones from a Clay Pit near North Haven. Amer. Jour. Sci. (3), vol. 10, pp. 354–355.

On the *Odontornithes*, or Birds with Teeth. *Ibid.*, vol. 10, pp. 403–408, pls. ix–x; Amer. Nat., vol. 9, pp. 625–631, pls. ii, iii; Jour. Zool., Paris, vol. 4, pp. 494–502, pl. xv; Geol. Mag. (2), vol. 3, pp. 49–53, pl. ii, 1876. Abstract: N. Jahrb., pp. 333–334, 1876.

1876

Principal Characters of the *Dinocerata*. Pt. I. Amer. Jour. Sci. (3), vol. 11, pp. 163–168, pls. ii–vi; Jour. Zool., Paris, vol. 5, pp. 136–145, pl. iv. Abstract: Zeits. Gesammt. Naturw., vol. 14, pp. 31–32; N. Jahrb., pp. 780–781; Pop. Sci. Rev., London, pp. 326–327; Nature, vol. 13, p. 374, March 9, 1876.

Principal Characters of the *Tillodontia*. Pt. I. Amer. Jour. Sci. (3), vol. 11, pp. 249–252, pls. viii–ix, 1 fig.; Jour. Zool., Paris, vol. 5, pp. 244–248, pl. xi. Abstract: N. Jahrb., p. 781; Nature, vol. 13, p. 374, March 9, 1876; Zeits. Gesammt. Naturw., vol. 14, pp. 32–33.

Principal Characters of the *Brontotheridae*. Amer. Jour. Sci. (3), vol. 11, pp. 335–340, pls. x–xiii, 2 figs.; Jour. Zool., Paris, vol. 5, pp. 248–255, pl. xii. Abstract: Nature, vol. 14, p. 36, May 11, 1876; Zeits. Gesammt. Naturw., vol. 14, pp. 33–34; N. Jahrb., p. 781.

On some Characters of the Genus *Coryphodon*, Owen. Amer. Jour. Sci. (3), vol. 11, pp. 425–428, 1 pl. Abstract: Pop. Sci. Rev., London, p. 327; N. Jahrb., p. 781; Archiv. Sci. Phys. Nat., Geneva, vol. 56, pp. 373–374.

Notice of a new Sub-order of *Pterosauria*. Amer. Jour. Sci. (3), vol. 11, pp. 507–509. Abstract: Ann. Mag. Nat. Hist. (4), vol. 18, pp. 195–196; Jour. Zool., Paris, vol. 5, pp. 457–458; N. Jahrb., pp. 781–782; Archiv. Sci. Phys. Nat., Geneva, vol. 56, pp. 374–375.

Notice of new *Odontornithes*. Amer. Jour. Sci. (3), vol. 11, pp. 509–511; Jour. Zool., Paris, vol. 5, pp. 304–306. Abstract: Pop. Sci. Rev., London, p. 327; N. Jahrb., p. 782; Archiv. Sci. Phys. Nat., Geneva, vol. 56, p. 373.

Recent Discoveries of Extinct Animals. Amer. Jour. Sci. (3), vol. 12, pp. 59–61; Amer. Nat., vol. 10, pp. 436–439. Abstract: N. Jahrb., p. 782.

Notice of new Tertiary Mammals. Pt. V. Amer. Jour. Sci. (3), vol. 12, pp. 401–404. Abstract: N. Jahrb., p. 557; Pop. Sci. Rev., London, pp. 97–98, 1877; Archiv. Sci. Phys. Nat., Geneva, vol. 59, pp. 127–128, 1877.

Principal Characters of American Pterodactyls. Amer. Jour. Sci. (3), vol. 12, pp. 479–480. Abstract: Pop. Sci. Rev., London, p. 97, 1877; N. Jahrb., pp. 557–558, 1877; Archiv. Sci. Phys. Nat., Geneva, vol. 59, pp. 126–127, 1877.

Articles on *Hadrosaurus, Hesperornis,* Horse, *Hyaena, Hyaenodon, Hylaeosaurus, Hyopsodus, Hyrax, Ichthyosaurus, Iguana,* and *Iguanodon.* Johnson's Cyclopaedia, vol. 2.

1877

Brain of *Coryphodon.* Amer. Nat., vol. 11, p. 375. Abstract: Nature, vol. 17, p. 340, Feb. 28, 1878.

Principal Characters of the *Coryphodontidae.* Amer. Jour. Sci. (3), vol. 14, pp. 81-85, pl. iv; Jour. Zool., vol. 6, pp. 380-385. Abstract: Amer. Nat., vol. 11, p. 500; N. Jahrb., p. 767.

Characters of the *Odontornithes,* with Notice of a new allied Genus. Amer. Jour. Sci. (3), vol. 14, pp. 85-87, pl. v; Jour. Zool., Paris, vol. 6, pp. 385-389. Abstract: N. Jahrb., p. 767.

Notice of a new and gigantic Dinosaur. Amer. Jour. Sci. (3), vol. 14, pp. 87-88; Jour. Zool., Paris, vol. 6, pp. 248-250. Abstract: N. Jahrb., pp. 767-768; Pop. Sci. Rev., London, p. 424.

Notice of some new Vertebrate Fossils. Amer. Jour. Sci. (3), vol. 14, pp. 249–256, 1 fig. Abstract: Amer. Nat., vol. 11, pp. 629–631.

Introduction and Succession of Vertebrate Life in America. Vice-President's Address, American Association for the Advancement of Science, Nashville meeting, August, 1877. Author's ed., 57 pp., New Haven, Aug., 1877; Nature, vol. 16, pp. 448-450, 470-472, 489-491, Sept. 20, 27, Oct. 4, 1877; Amer. Jour. Sci. (3), vol. 14, pp. 337-378, Nov., 1877; Pop. Sci. Mo., vol. 12, pp. 513-527, 672-697, 1 pl., 1878; Rev. Scientif., Paris (2), vol. 7, pp. 1039-1046, 1064-1074, May 4, 11, 1878. Abstract: N. Jahrb., pp. 442-444, 1878.

A new Order of Extinct *Reptilia* (*Stegosauria*), from the Jurassic of the Rocky Mountains. Amer. Jour. Sci. (3), vol. 14, pp. 513-514. Abstract: Pop. Sci. Rev., London, p. 103, 1878.

Notice of new Dinosaurian Reptiles from the Jurassic Formation. Amer. Jour. Sci. (3), vol. 14, pp. 514-516. Abstract: Pop. Sci. Rev., London, pp. 103-104, 1878.

Articles on *Lophiodon, Loxodon, Machaerodus,* Macrauchenidae, Mammoth, Mastodon, *Megaceros, Megalosaurus, Megatherium,* Mosasauridae, Odontornithes, Pterodactyl, and Quadrumana. Johnson's Cyclopaedia, vol. 3.

Speech at 72d Anniversary Celebration, New England Society in New York, Dec. 22, 1877. Abstract: [Ann. Rept.], p. 59.

1878

New Species of *Ceratodus,* from the Jurassic. AMER. JOUR. SCI. (3), vol. 15, p. 76, 1 fig.; ANN. MAG. NAT. HIST. (5), vol. 1, p. 184. Abstract: POP. SCI. REV., London, p. 210; SCI. GOSSIP, vol. 14, p. 117.

Notice of new Dinosaurian Reptiles. AMER. JOUR. SCI. (3), vol. 15, pp. 241-244, figs. 1, 2. Abstract: POP. SCI. REV., London, pp. 210-211.

Notice of new Fossil Reptiles. AMER. JOUR. SCI. (3), vol. 15, pp. 409-411. Abstract: N. JAHRB., p. 777; POP. SCI. REV., London, p. 322.

Fossil Mammal from the Jurassic of the Rocky Mountains. AMER. JOUR. SCI. (3), vol. 15, p. 459; POP. SCI. REV., London, pp. 322-323; ANN. MAG. NAT. HIST. (5), vol. 2, p. 108. Abstract: N. JAHRB., p. 779.

New Pterodactyl from the Jurassic of the Rocky Mountains. AMER. JOUR. SCI. (3), vol. 16, pp. 233-234. Abstract: N. JAHRB., p. 895; POP. SCI. REV., London, p. 436.

Principal Characters of American Jurassic Dinosaurs. Pt. I. AMER. JOUR. SCI. (3), vol. 16, pp. 411-416, pls. iv-x. Abstract: N. JAHRB., pp. 256-257, 1880; KOSMOS, vol. 5, pp. 137-139, 1879.

Scientific Museums. Address at opening of American Museum of Natural History, Dec. 22, 1877. 9th ANN. REPT. AMER. MUS. NAT. HIST., pp. 52–54; POP. SCI. MO., vol. 12, pp. 475–476.

Articles on *Coryphodon, Eohippus, Pteranodon, Sivatherium,* Tillodontia, *Tinoceras,* and *Titanotherium.* Johnson's Cyclopaedia, vol. 4 and Appendix.

1879

A new Order of Extinct Reptiles (*Sauranodonta*), from the Jurassic Formation of the Rocky Mountains. AMER. JOUR. SCI. (3), vol. 17, pp. 85–86. Abstract: ANN. MAG. NAT. HIST. (5), vol. 3, pp. 175–176; POP. SCI. REV., London, p. 205; N. JAHRB., p. 256, 1880; KOSMOS, vol. 5, pp. 139–140.

Principal Characters of American Jurassic Dinosaurs. Pt. II. AMER. JOUR. SCI. (3), vol. 17, pp. 86–92, pls. iii–x. Abstract: N. JAHRB., pp. 257–258, 1880.

Additional Characters of the *Sauropoda.* AMER. JOUR. SCI. (3), vol. 17, pp. 181–182.

The Vertebrae of recent Birds. *Ibid.,* vol. 17, pp. 266–269, figs. 1–5.

Polydactyl Horses, recent and extinct. *Ibid.,* vol 17, pp. 499–505, 1 pl., figs. 1–4. Abstract: POP. SCI. REV., London, pp. 318–319; N. JAHRB., pp. 103–104, 1881; KOSMOS, vol. 5, pp. 432–438.

Notice of a new Jurassic Mammal. Amer. Jour. Sci. (3), vol. 18, pp. 60–61. Ann. Mag. Nat. Hist. (5), vol. 4, pp. 167–168; Geol. Mag. (2), vol. 6, pp. 371–372. Abstract: Pop. Sci. Rev., London, p. 428; N. Jahrb., p. 217, 1880.

Address in Reply to Mayor of St. Louis, American Association for the Advancement of Science, St. Louis meeting. Proc. Amer. Assoc. Adv. Sci., 1878, vol. 27, pp. 350–351.

Additional Remains of Jurassic Mammals. Amer. Jour. Sci. (3), vol. 18, pp. 215–216, 1 fig. Abstract: Pop. Sci. Rev., London, p. 428; N. Jahrb., p. 216, 1880.

History and Methods of Palaeontological Discovery. President's Address, American Association for the Advancement of Science, Saratoga meeting, August, 1879. Author's ed., 51 pp., New Haven, Aug., 1879; Nature, vol. 20, pp. 494–499, 515–521, Sept. 18, 25, 1879; Amer. Jour. Sci. (3), vol. 18, pp. 323–359, Nov., 1879; Pop. Sci. Mo., vol. 16, pp. 219–236, 363–380, 1879–1880; Kosmos, vol. 6, pp. 339–352, 425–445; Proc. Amer. Assoc. Adv. Sci., 1879, vol. 28, pp. 1–42, 1880.

Notice of new Jurassic Mammals. Amer. Jour. Sci. (3), vol. 18, pp. 396–398, 1 fig. Abstract: Ann. Mag. Nat. Hist. (5), vol. 5, p. 84, 1880; N. Jahrb., pp. 217–218, 1880.

Notice of new Jurassic Reptiles. Amer. Jour. Sci. (3), vol. 18, pp. 501–505, pl. iii, 1 fig. Abstract: N. Jahrb., p. 108, 1880.

Peabody Museum. Yale Book, vol. 2, pp. 178-186.

Report of National Academy of Sciences for 1878. Forty-sixth Congress, 1st Sess., House of Representatives, Misc. Doc. No. 7, 25 pp., Washington.

1880

New Characters of Mosasauroid Reptiles. Amer. Jour. Sci. (3), vol. 19, pp. 83–87, pl. i, figs. 1–4. Abstract: N. Jahrb., pp. 104–105.

The Limbs of *Sauranodon*, with Notice of a new Species. Amer. Jour. Sci. (3), vol. 19, pp. 169–171, 1 fig. Abstract: N. Jahrb., pp. 105–106; Kosmos, vol. 7, pp. 74–77.

Principal Characters of American Jurassic Dinosaurs. Pt. III. Amer. Jour. Sci. (3), vol. 19, pp. 253–259, pls. vi–xi, 1 fig. Abstract: N. Jahrb., pp. 106–107; Kosmos, vol. 7, pp. 213–215.

The Sternum in Dinosaurian Reptiles. Amer. Jour. Sci. (3), vol. 19, pp. 395–396, pl. xviii. Abstract: N. Jahrb., pp. 104–105, 1881; Kosmos, vol. 7, pp. 317–318.

Note on *Sauranodon*. Amer. Jour. Sci. (3), vol. 19, p. 491.

Odontornithes: a Monograph on the Extinct Toothed Birds of North America. With 34 plates and 40 woodcuts. 4°, xv + 201 pp. U. S. Geol. Exploration 40th Parallel, vol. 7, Washington; Mem. Peabody Mus., Yale Coll., vol. 1. Abstract: Nature, vol. 22, pp. 457–458, Sept. 16, 1880; Geol. Mag. (2), vol. 7, pp. 522–526; Amer. Jour. Sci. (3), vol. 21, pp. 255–276, 1881; Science, vol. 2, pp. 98–99, 147–151, March 5, April 2, 1881; Archiv. Sci. Phys. Nat., Geneva,

pp. 409–430, 1881; KOSMOS, vol. 9, pp. 157–164, 1881; N. JAHRB., pp. 270–273, 1881.

Notice of Jurassic Mammals representing two new Orders. AMER. JOUR. SCI. (3), vol. 20, pp. 235-239, figs. 1, 2. Abstract: ARCHIV. SCI. PHYS. NAT., Geneva, vol. 4, pp. 420–422.

List of Genera established by Prof. O. C. Marsh, 1862–1879. Pp. 12, New Haven.

1881

Principal Characters of American Jurassic Dinosaurs. Pt. IV. Spinal Cord, Pelvis, and Limbs of *Stegosaurus*. AMER. JOUR. SCI. (3), vol. 21, pp. 167–170, pls. vi–viii. Abstract: N. JAHRB., pp. 109–110, 270–271; KOSMOS, vol. 9, pp. 319–321, 5 figs.

A new Order of Extinct Jurassic Reptiles (*Coeluria*). AMER. JOUR. SCI. (3), vol. 21, pp. 339–340, pl. x; KOSMOS, vol. 9, pp. 464–465. Abstract: N. JAHRB., pp. 414–415.

Discovery of a Fossil Bird in the Jurassic of Wyoming. AMER. JOUR. SCI. (3), vol. 21, pp. 341–342. ANN. MAG. NAT. HIST. (5), vol. 7, pp. 488–489. Abstract: N. JAHRB., p. 414.

Note on American Pterodactyls. AMER. JOUR. SCI. (3), vol. 21, pp. 342–343. Abstract: N. JAHRB., p. 415.

Principal Characters of American Jurassic Dinosaurs. Pt. V. AMER. JOUR. SCI. (3), vol. 21, pp. 417–423, pls. xii–xviii. Abstract: N. JAHRB., pp. 415–416; KOSMOS, vol. 9, pp. 465–466.

Notice of new Jurassic Mammals. AMER. JOUR. SCI. (3), vol. 21, pp. 511–513.

Restoration of *Dinoceras mirabile*. *Ibid.*, vol. 22, pp. 31–32, pl. ii. Abstract: ARCHIV. SCI. PHYS. NAT., Geneva, vol. 6, pp. 323–324.

Jurassic Birds and their Allies. SCIENCE, vol. 2, pp. 512–513; AMER. JOUR. SCI. (3), vol. 22, pp. 337–340; GEOL. MAG. (2), vol. 8, pp. 485–487; NATURE, vol. 25, pp. 22–23, Nov. 3, 1881; ARCHIV. SCI. PHYS. NAT., Geneva, vol. 7, pp. 312–318, 1882; ANN. MAG. NAT. HIST. (5), vol. 8, pp. 452–455; KOSMOS, vol. 10, pp. 231–234; POP. SCI. MO., vol. 20, pp. 312–315, 1882; REPT. BRIT. ASSOC. ADV. SCI., 51ST MEETING, YORK, pp. 661–662. Abstract: NATURE, vol. 24, p. 501, Sept. 22, 1881; REV. ZOOL. ET PAL., p. 409, 1882.

1882

Classification of the *Dinosauria*. AMER. JOUR. SCI. (3), vol. 23, pp. 81–86; KOSMOS, vol. 10, pp. 382–387; NATURE, vol. 25, pp. 244–246, Jan. 12, 1882; GEOL. MAG. (2), vol. 9, pp. 80–85, 1882; ANN. MAG. NAT. HIST. (5), vol. 9, pp. 79–84, 1882. Abstract: POP. SCI. REV., London, pp. 272–273, 1881.

The Wings of Pterodactyles. AMER. JOUR. SCI. (3), vol. 23, pp. 251–256, pl. iii, figs. 1, 2; NATURE, vol. 25, pp. 531–533, Apr. 6, 1882; GEOL. MAG. (2), vol. 9, pp. 205–210; KOSMOS, vol. 11, pp. 103–108. Abstract: N. JAHRB., vol. 2, p. 207, 1883.

Evolution. Address delivered at the farewell dinner to Herbert Spencer, in New York, Nov. 9, 1882, pp. 4, New York; also in "Herbert Spencer and the Americans, and the Americans and Herbert Spencer", pp. 45–50, New York, 1883.

List of Scientific Publications of O. C. Marsh, 1861–1881. Pp. 29, New Haven.

1883

Birds with Teeth. *3rd Ann. Rept. Director U. S. Geol. Surv.*, pp. 45–88, 33 figs.

Principal Characters of American Jurassic Dinosaurs. Pt. VI. Restoration of *Brontosaurus*. Amer. Jour. Sci. (3), vol. 26, pp. 81–85, pl. i; Geol. Mag. (2), vol. 10, pp. 386–388, pl. ix. Abstract: N. Jahrb., vol. 2, pp. 104–105, 1884; Kosmos, vol. 13, pp. 549–552.

On the supposed Human Footprints recently found in Nevada. Amer. Jour. Sci. (3), vol. 26, pp. 139–140, figs. 1, 2. Abstract: Nature, vol. 28, pp. 370–371, Aug. 16, 1883; N. Jahrb., vol. 1, pp. 262–263, 1884.

Report of National Academy of Sciences for 1882. Pp. 16, Washington.

1884

Principal Characters of American Jurassic Dinosaurs. Pt. VII. On the *Diplodocidae*, a new Family of the *Sauropoda*. Amer. Jour. Sci. (3), vol. 27, pp. 160-167, pls. iii-iv; Geol. Mag. (3), vol. 1, pp. 99-107, 10 figs. Abstract: N. Jahrb., vol. 1, p. 88, 1885; Science, vol. 3, p. 199, 3 figs.; Kosmos, vol. 14, pp. 350-357.

Principal Characters of American Jurassic Dinosaurs. Pt. VIII. The Order *Theropoda*. Amer. Jour. Sci. (3), vol. 27, pp. 329-340, pls. viii-xiv; Geol. Mag. (3), vol. 1, pp. 252-262. Abstract: Science, vol. 3, pp. 542-544, figs., May 2, 1884; Nature, vol. 30, pp. 201-202, June 26, 1884; Kosmos, vol. 14, pp. 357-365; N. Jahrb., vol. 1, pp. 89-90, 1885.

A new Order of Extinct Jurassic Reptiles (*Macelognatha*). Amer. Jour. Sci. (3), vol. 27, p. 341, 1 fig. Abstract: N. Jahrb., vol. 1, p. 473, 1885.

Principal Characters of American Cretaceous Pterodactyls. Pt. I. The Skull of *Pteranodon*. Amer. Jour. Sci. (3), vol. 27, pp. 423–426, pl. xv; Geol. Mag. (3), vol. 1, pp. 345-348. Abstract: N. Jahrb., vol. 1, p. 472, 1885.

On the united Metatarsal Bones of *Ceratosaurus*. Amer. Jour. Sci. (3), vol. 28, pp. 161–162, figs. 1, 2. Abstract: Kosmos, vol. 16, pp. 372–373, 1885; N. Jahrb., vol. 1, p. 90, 1885.

On the Classification and Affinities of Dinosaurian Reptiles. Nature, vol. 31, pp. 68-69, Nov. 20, 1884; Rept. Brit. Assoc. Adv. Sci., Montreal meeting, pp. 763-766, 1885. Abstract: Science, vol. 4, p. 261, Sept. 12, 1884.

Administrative Report to the Director of the U. S. Geological Survey. Fourth Ann. Rept. U. S. Geol. Surv., pp. 41-42.

Report of the National Academy of Sciences for 1883. Pp. 143, Washington.

Acting President's Address, National Academy of Sciences, Washington, April 15, 1879. Proc. Nat. Acad. Sci., vol. 1, pp. 148-153.

1885

The gigantic Mammals of the Order *Dinocerata*. Fifth Ann. Rept. U. S. Geol. Surv., pp. 243-302, figs. 36-137.

On American Jurassic Mammals. Rept. Brit. Assoc. Adv. Sci., Montreal meeting, 1884, pp. 734-736.

Names of Extinct Reptiles. Amer. Jour. Sci. (3), vol. 29, p. 169.

On the Size of the Brain in Extinct Animals. Abstract: Nature, vol. 32, p. 562, Oct. 8, 1885; Science, vol. 6, p. 360, Oct. 23, 1885; Rept. Brit. Assoc. Adv. Sci., Aberdeen meeting, 1885, p. 1065, 1886.

Report of National Academy of Sciences for 1884. Pp. 67, Washington.

Administrative Report to the Director of the U. S. Geological Survey. Fifth Ann. Rept. U. S. Geol. Surv., pp. 49-50.

Administrative Report to the Director of the U. S. Geological Survey. Sixth Ann. Rept. U. S. Geol. Surv., pp. 71-72.

Dinocerata: a Monograph of an Extinct Order of Gigantic Mammals. With 56 plates and 200 woodcuts. 4°, xviii + 237 pp. Monographs U. S. Geol. Surv., vol. 10, Washington, 1886. (Author's edition, title page dated 1884; published 1885.) Abstract: Amer. Jour. Sci. (3), vol. 29, pp. 173–204; Geol. Mag. (3), vol. 2, pp. 212–228; Nature, vol. 32, pp. 97–99, June 4, 1885; Pop. Sci. Mo., vol. 28, pp. 133–134; N. Jahrb., vol. 1, pp. 339–341, 1886; Ann. Sci. Géol., Paris, vol. 17, pp. 1–11, 1884.

1886

Report of the National Academy of Sciences for 1885. Pp. 101, Washington.

1887

American Jurassic Mammals. Amer. Jour. Sci. (3), vol. 33, pp. 327-348, pls. vii-x; Geol. Mag. (3), vol. 4, pp. 241-247, 289-299, pls. vi-ix. Abstract: Naturwiss. Rundschau, Braunschweig, vol. 2, p. 362; N. Jahrb., vol. 1, pp. 106-108, 1888.

Notice of new Fossil Mammals. Amer. Jour. Sci. (3), vol. 34, pp. 323-331, figs. 1-12.

Principal Characters of American Jurassic Dinosaurs. Part IX. The Skull and Dermal Armor of *Stegosaurus*. *Ibid.*, vol. 34, pp. 413-417, pls. vi-ix; Geol. Mag. (3), vol. 5, pp. 11-15, pls. i-iii, 3 figs., 1888.

Report of the National Academy of Sciences for 1886. Pp. 40, Washington.

Additional Genera established by Professor O. C. Marsh, 1880-1887. Pp. 13-15, New Haven.

1888

Notice of a new Genus of *Sauropoda* and other new Dinosaurs from the Potomac Formation. AMER. JOUR. SCI. (3), vol. 35, pp. 89-94, figs. 1-9.

Notice of a new Fossil Sirenian, from California. *Ibid.*, vol. 35, pp. 94-96, figs. 1-3.

A new Family of Horned *Dinosauria*, from the Cretaceous. *Ibid.*, vol. 36, pp. 447-478, pl. xi.

Report of the National Academy of Sciences for 1887. Pp. 55, Washington.

List of Scientific Publications of Othniel Charles Marsh. Part II. 1882-1887. Pp. 5, New Haven.

Administrative Report to the Director of the U. S. Geological Survey. SEVENTH ANN. REPT. U. S. GEOL. SURV., pp. 111-113.

1889

Restoration of *Brontops robustus*, from the Miocene of America. Abstract: AMER. JOUR. SCI. (3), vol. 37, pp. 163-165, pl. vi; REPT. BRIT. ASSOC. ADV. SCI., BATH MEETING, 1888, pp. 706-707; GEOL. MAG. (3), vol. 6, pp. 99-101, pl. iv.

Comparison of the principal Forms of the *Dinosauria* of Europe and America. Abstract: AMER. JOUR. SCI. (3), vol. 37, pp. 323-331; GEOL. MAG. (3), vol. 6, pp. 204-210.

Notice of new American *Dinosauria*. AMER. JOUR. SCI. (3), vol. 37, pp. 331-336, figs. 1-5. Abstract: N. JAHRB., vol. 2, p. 434, 1890.

Discovery of Cretaceous *Mammalia*. AMER. JOUR. SCI. (3), vol. 38, pp. 81-92, pls. ii-v.

Notice of gigantic Horned *Dinosauria* from the Cretaceous. *Ibid.*, vol. 38, pp. 173-175, 1 fig. Abstract: N. JAHRB., vol. 2, p. 435, 1890.

Discovery of Cretaceous *Mammalia*. Pt. II. AMER. JOUR. SCI. (3), vol. 38, pp. 177-180, pls. vii-viii.

The Skull of the gigantic *Ceratopsidae*. Abstract: *Ibid.*, vol. 38, pp. 501-506, pl. xii; GEOL. MAG. (3), vol. 7, pp. 1-5, pl. i, 1890.

Administrative Report to the Director of the U. S. Geological Survey. EIGHTH ANN. REPT. U. S. GEOL. SURV., pp. 173-174.

Administrative Report to the Director of the U. S. Geological Survey. NINTH ANN. REPT. U. S. GEOL. SURV., pp. 114-115.

1890

Description of new Dinosaurian Reptiles. AMER. JOUR. SCI. (3), vol. 39, pp. 81-86, pl. i, figs. 1, 2.

Distinctive Characters of the Order *Hallopoda*. *Ibid.*, vol. 39, pp. 415-417, 1 fig.

Additional Characters of the *Ceratopsidae*, with notice of new Cretaceous Dinosaurs. *Ibid.*, vol. 39, pp. 418-426, pls. v-vii, figs. 1-3.

Notice of new Tertiary Mammals. *Ibid.*, vol. 39, pp. 523-525.

Notice of some Extinct *Testudinata*. *Ibid.*, vol. 40, pp. 177-179, pls. vii-viii.

Reply to Professor Cope. NEW YORK HERALD, Jan. 19, 1890; privately printed, 23 pp., New Haven, 1894.

Report of the National Academy of Sciences for 1888. Pp. 41, Washington.

Additional Genera established by Professor O. C. Marsh. 1880-1889. Pp. 5, New Haven.

Administrative Report to the Director of the U. S. Geological Survey. TENTH ANN. REPT. U. S. GEOL. SURV., pp. 158-159.

1891

A Horned Artiodactyle (*Protoceras celer*) from the Miocene. AMER. JOUR. SCI. (3), vol. 41, pp. 81–82.

On the gigantic *Ceratopsidae*, or Horned Dinosaurs, of North America. *Ibid.*, vol. 41, pp. 167–178, pls. i–x; GEOL. MAG. (3), vol. 8, pp. 193–199, figs. 1–19, pls. iv, v, pp. 241–248, figs. 1–34. Abstract: REPT. BRIT. ASSOC. ADV. SCI., LEEDS MEETING, 1890, pp. 793–795.

On the Cretaceous Mammals of North America. Abstract: REPT. BRIT. ASSOC. ADV. SCI., LEEDS MEETING, 1890, pp. 853–854.

Restoration of *Triceratops* [and *Brontosaurus*]. AMER. JOUR. SCI. (3), vol. 41, pp. 339–342, pls. xv–xvi. Abstract: GEOL. MAG. (3), vol. 8, pp. 248–250, pl. vii.

Note on Mesozoic Mammalia. AMER. NAT., vol. 25, pp. 611–616; privately printed, 5 pp., New Haven. Abstract: PROC. ACAD. NAT. SCI. PHILADELPHIA, vol. 43, part II, pp. 237–241.

Restoration of *Stegosaurus*. AMER. JOUR. SCI. (3), vol. 42, pp. 179–181, pl. ix; GEOL. MAG. (3), vol. 8, pp. 385–387, pl. xi.

Notice of new Vertebrate Fossils. AMER. JOUR. SCI. (3), vol. 42, pp. 265–269.

Geological Horizons as determined by Vertebrate Fossils. Abstract: *Ibid.*, vol. 42, pp. 336–338, pl. xii; privately printed, 3 pp., New Haven; COMPT. REND. CONG. GÉOL. INTERNAT., 5TH SESS., WASHINGTON, pp. 156–159, pl. (p. 157), 1893.

Report of the National Academy of Sciences for 1889. Pp. 69, Washington.

Report of the National Academy of Sciences for 1890. Pp. 36, Washington.

Administrative Report to the Director of the U. S. Geological Survey. ELEVENTH ANN. REPT. U. S. GEOL. SURV., Part I, pp. 101–102.

Administrative Report to the Director of the U. S. Geological Survey. TWELFTH ANN. REPT. U. S. GEOL. SURV., Part I, pp. 118–119.

Statement relating to the Founding of the Peabody Museum at Harvard. TWENTY-FIFTH ANN. REPT. PEABODY MUS. AMER. ARCHAEOL. ETHNOL., p. 1.

1892

The Skull of *Torosaurus*. AMER. JOUR. SCI. (3), vol. 43, pp. 81–84, pls. ii–iii.

Discovery of Cretaceous *Mammalia*. Pt. III. *Ibid.*, vol. 43, pp. 249–262, pls. v–xi.

Recent [and extinct] polydactyle Horses. *Ibid.*, vol. 43, pp. 339–355, figs. 1–21 and diagram.

A new Order of Extinct Eocene Mammals (*Mesodactyla*). *Ibid.*, vol. 43, pp. 445–449, figs. 1, 2.

Notice of new Reptiles from the Laramie Formation. *Ibid.*, vol. 43, pp. 449–453, figs. 1–4.

Notes on Triassic *Dinosauria*. *Ibid.*, vol. 43, pp. 543–546, pls. xv–xvii.

Notes on Mesozoic Vertebrate Fossils. *Ibid.*, vol. 44, pp. 171–176, pls. ii–v.

Restorations of *Claosaurus* and *Ceratosaurus*. *Ibid.*, vol. 44, pp. 343–349, pls. vi–vii.

Restoration of *Mastodon americanus*, Cuvier. *Ibid.*, vol. 44, p. 350, pl. viii; GEOL. MAG. (3), vol. 10, p. 164, pl. viii, 1893.

Report of the National Academy of Sciences for 1891. Pp. 39, Washington.

Administrative Report to the Director of the U. S. Geol. Surv. THIRTEENTH ANN. REPT. U. S. GEOL. SURV., Part I, pp. 155–157.

1893

A new Cretaceous Bird allied to *Hesperornis*. AMER. JOUR. SCI. (3), vol. 45, pp. 81–82, figs. 1–3.

The Skull and Brain of *Claosaurus*. *Ibid.*, vol. 45, pp. 83–86, pls. iv–v.

Restoration of *Anchisaurus*. *Ibid.*, vol. 45, pp. 169–170, pl. vi.

Restorations of *Anchisaurus, Ceratosaurus*, and *Claosaurus*. GEOL. MAG. (3), vol. 10, pp. 150–157, pls. vi, vii, fig. 1.

Some recent Restorations of Dinosaurs. NATURE, vol. 48, pp. 437–438, Sept. 7, 1893; privately printed, 3 pp., New Haven.

Restoration of *Coryphodon*. AMER. JOUR. SCI. (3), vol. 46, pp. 321–326, pls. v–vi; GEOL. MAG. (3), vol. 10, pp. 481–487, figs. 1–6, pl. xviii.

Description of Miocene *Mammalia*. AMER. JOUR. SCI. (3), vol. 46, pp. 407–412, pls. vii–x.

Scientific Publications of Othniel Charles Marsh, 1861–1892. *Bibliographies of the Present Officers of Yale University*, pp. 88–102.

Report of the National Academy of Sciences for 1892. Pp. 39, Washington.

Administrative Report to the Director of the U. S. Geological Survey. FOURTEENTH ANN. REPT. U. S. GEOL. SURV., Part I, pp. 265–267.

1894

Restoration of *Camptosaurus*. AMER. JOUR. SCI. (3), vol. 47, pp. 245–246, pl. vi; GEOL. MAG. (4), vol. 1, pp. 193–195, pl. vi.

Restoration of *Elotherium*. AMER. JOUR. SCI. (3), vol. 47, pp. 407–408, pl. ix; GEOL. MAG. (4), vol. 1, pp. 294–295, pl. x.

A new Miocene Mammal. AMER. JOUR. SCI. (3), vol. 47, p. 409, 3 figs.

Footprints of Vertebrates in the Coal Measures of Kansas. *Ibid.*, vol. 48, pp. 81–84, pls. ii–iii; SCI. AMER. SUPPL., No. 969, pp. 15491–15492,

figs., July 28, 1894. Abstract: GEOL. MAG. (4), vol. 1, pp. 337–339, 432, pl. xi.

The Typical *Ornithopoda* of the American Jurassic. AMER. JOUR. SCI., vol. 48, pp. 85–90, pls. iv–vii.

Eastern Division of the Miohippus Beds, with Notes on some of the Characteristic Fossils. *Ibid.*, vol. 48, pp. 91–94, figs. 1, 2.

Miocene Artiodactyles from the eastern Miohippus Beds. *Ibid.*, vol. 48, pp. 175–178, figs. 1–7.

Description of Tertiary Artiodactyles. *Ibid.*, vol. 48, pp. 259–274, figs. 1–34.

A gigantic Bird from the Eocene of New Jersey. *Ibid.*, vol. 48, p. 344, 5 figs.

A new Miocene Tapir. *Ibid.*, vol. 48, p. 348.

1895

On the *Pithecanthropus erectus*, Dubois, from Java. *Ibid.*, vol. 49, pp. 144-147, pl. ii, fig. 2.

National Academy of Sciences on Electrical Measurement. *Ibid.*, vol. 49, pp. 236–237.

Thomas Henry Huxley. *Ibid.*, vol. 50, pp. 177–183.

The *Reptilia* of the Baptanodon Beds. *Ibid.*, vol. 50, pp. 405-406, figs. 1–3.

Restorations of some European Dinosaurs, with Suggestions as to their place among the *Reptilia*. Abstract: *Ibid.*, pp. 407–412, pls. v–viii, 1 fig.; REPT. BRIT. ASSOC. ADV. SCI., IPSWICH MEETING, 1895, pp. 685–688; GEOL. MAG. (4), vol. 3, pp. 1–9, pls. i–iv, figs. 1, 2, 1896.

On the Affinities and Classification of the Dinosaurian Reptiles. Abstract: AMER. JOUR. SCI. (3), vol. 50, pp. 483–498, pl. x, figs. 1–11; reprinted with alterations under the title "Classification of Dinosaurs," GEOL. MAG. (4), vol. 3, pp. 388–400, figs. 1-12, 1896; COMPT. REND. CONG. INTERNAT. ZOOL., 31ST SESS., LEYDEN, 1895, pp. 196–211, pl. i, figs. 1–11, 1896.

Fossil Vertebrates. JOHNSON'S UNIVERSAL CYCLOPAEDIA, new ed., vol. 8, pp. 491–498, 1 pl.

Note on Globular Lightning. NATURE, vol. 53, p. 152, Dec. 19, 1895; AMER. JOUR. SCI. (4), vol. 1, pp. 13–14, 1896.

Letter to the President of the Senate, transmitting the Report of the National Academy of Sciences on Electrical Units. FIFTY-THIRD CONGR., 3D SESSION, Senate Misc. Doc. No. 115, p. 1.

Report of the National Academy of Sciences for 1893. Pp. 47, Washington.

Report of the National Academy of Sciences for 1894. Pp. 43, Washington.

Address delivered before the National Academy of Sciences, Washington, April 19, 1889. PROC. NAT. ACAD. SCI., vol. 1, pp. 325–330.

Address as Retiring President, delivered before the National Academy of Sciences, Washington, April 19, 1895. Privately printed, 6 pp., New Haven. Printed by the Academy, 6 pp., Washington.

Restorations of extinct Animals. Plate I. Privately printed, New Haven.

Restorations of Dinosaurian Reptiles. Plate II. Privately printed, New Haven.

1896

The Age of the Wealden. Amer. Jour. Sci. (4), vol. 1, p. 234; Nature, vol. 53, p. 436, March 12, 1896.

On the *Pithecanthropus erectus,* from the Tertiary of Java. Abstract: Amer. Jour. Sci. (4), vol. 1, pp. 475-482, pl. xiii, 4 figs.; reprinted under the title "The Apeman from the Tertiary of Java," Science, vol. 3, pp. 789-793, figs. 1-4; Album der Natur, Haarlem, pp. 270-274, 276-280.

A new Belodont Reptile (*Stegomus*) from the Connecticut River Sandstone. Amer. Jour. Sci. (4), vol. 2, pp. 59-62, pl. i, figs. 1-3.

The Geology of Block Island. *Ibid.*, vol. 2, pp. 295-298, 375-377.

Amphibian Footprints from the Devonian. *Ibid.*, vol. 2, pp. 374-375, 1 fig.

The Dinosaurs of North America. 16th Ann. Rept. U. S. Geol. Surv., Part I, pp. 133-244[414], pls. ii-lxxxv, figs. 1-66. Abstract: Geol. Mag. (4), vol. 4, pp. 38-44.

The Jurassic Formation on the Atlantic Coast. Abstract: Amer. Jour. Sci. (4), vol. 2, pp. 433-447, figs. 1, 2; Science, n. s., vol. 4, pp. 805-816, figs. 1, 2, Dec. 4, 1896.

Vertebrate Fossils [of the Denver Basin]. Monographs U. S. Geol. Surv., vol. 27, pp. 473-527[550], pls. xxi-xxxi, figs. 23-102.

List of Scientific Publications of Othniel Charles Marsh. Part III. 1888-1896. Pp. 13, New Haven.

1897

The *Stylinodontia,* a Suborder of Eocene Edentates. Amer. Jour. Sci. (4), vol. 3, pp. 137-146, figs. 1-9.

The Affinities of *Hesperornis. Ibid.,* vol. 3, pp. 347-348; Nature, vol. 55, p. 534, April 8, 1897; Geol. Mag. (4), vol. 5, pp. 38-39, 1898.

Principal Characters of the *Protoceratidae.* Amer. Jour. Sci. (4), vol. 4, pp. 165-176, pls. ii-vii, figs. 1-7.

The Skull of *Protoceras.* Geol. Mag. (4), vol. 4, pp. 433-439, pl. xix, figs. 1, 2.

Recent Observations on European Dinosaurs. Amer. Jour. Sci. (4), vol. 4, pp. 413-416; Geol. Mag. (4), vol. 5, pp. 6-9, 1898.

List of Scientific Publications of Othniel Charles Marsh. Supplement. P. 1, New Haven.

1898

New Species of *Ceratopsia.* Amer. Jour. Sci. (4), vol. 6, p. 92.

The Jurassic Formation on the Atlantic Coast—Supplement. *Ibid.*, vol. 6, pp. 105-115, 1 fig.; (with additions) Science, n. s., vol. 8, pp. 145-154, 1 fig., Aug. 5, 1898.

Cycad Horizons in the Rocky Mountain Region. Amer. Jour. Sci. (4), vol. 6, p. 197; Science, n. s., vol. 8, pp. 153-154, Aug. 5, 1898.

The Value of Type Specimens and Importance of their Preservation. Amer. Jour. Sci. (4), vol. 6, pp. 401-405; Proc. 4th Internat. Cong. Zool., Cambridge, 1898, pp. 158-162, 1899.

The Origin of Mammals. AMER. JOUR. SCI. (4), vol. 6, pp. 406-409; SCIENCE, n. s., vol. 8, pp. 953-955, Dec. 30, 1898; GEOL. MAG. (4), vol. 6, pp. 13-16, 1899; PROC. 4TH INTERNAT. CONG. ZOOL., CAMBRIDGE, 1898, pp. 71-74, 1899.

The comparative Value of different Kinds of Fossils in determining geological Age. Abstract: AMER. JOUR. SCI. (4), vol. 6, pp. 483-486; GEOL. MAG. (4), vol. 5, pp. 565-568; REPT. BRIT. ASSOC. ADV. SCI., BRISTOL MEETING, 1898, pp. 869-872, 1899.

On the Families of Sauropodous *Dinosauria.* Abstract: AMER. JOUR. SCI. (4), vol. 6, pp. 487-488; REPT. BRIT. ASSOC. ADV. SCI., BRISTOL MEETING, 1898, pp. 909-910, 1899; GEOL. MAG. (4), vol. 6, pp. 157-158, 1899.

Presentation of Scientific Collections to Yale University. YALE ALUMNI WEEKLY, vol. 7, pp. 1, 4, Jan. 20, 1898; SCIENCE, n. s., vol. 7, pp. 77-79, Jan. 21, 1898. Abstract: REPT. PRESIDENT OF YALE UNIV. FOR 1897, pp. 74-79; AMER. JOUR. SCI. (4), vol. 5, pp. 156-157.

Informal Remarks at the Yale Alumni Dinner in New York, Feb. 14, 1898. NEW HAVEN REGISTER, p. 11, Feb. 20, 1898; YALE ALUMNI WEEKLY, vol. 7, p. 7, Feb. 24, 1898.

The Vertebrate Paleontology of the United States Geological Survey and National Museum. Privately printed, 12 pp., New Haven (1st and 2d ed.).

Vertebrate Fossils collected for the U. S. Geological Survey, 1882-1892. Pp. 4, New Haven.

Important Vertebrate Fossils for the National Museum. AMER. JOUR. SCI. (4), vol. 6, p. 101; SCIENCE, n. s., vol. 8, p. 43, July 8, 1898; AMER. GEOL., vol. 22, pp. 63-64. Abstract: NAT. SCI., vol. 13, p. 144.

The Proposed New Museum Building for Yale. YALE ALUMNI WEEKLY, vol. 7, pp. 1, 3, 16, July 12, 1898.

1899

Footprints of Jurassic Dinosaurs. AMER. JOUR. SCI. (4), vol. 7, pp. 227-232, pl. v, 3 figs.

Note on a Bridger Eocene Carnivore. *Ibid.,* vol. 7, p. 397.

Reply for American Delegation at Opening of International Congress of Zoology, Cambridge, England, 1898. PROC. 4TH INTERNAT. CONG. ZOOL., pp. 49-50, 1899.

BIOGRAPHICAL SKETCHES OF O. C. MARSH

Sketch of Professor O. C. Marsh, by G. B. Grinnell. POP. SC. MO., Sept., 1878, pp. 612–617.

Professor Marsh, by W. H. Bidwell. ECLECTIC MAG., April, 1878, pp. 501–502.

Prof. O. C. Marsh, M. A. THE PHILO MIRROR, Phillips Academy, Andover, June, 1884, pp. 28–30.

Othniel Charles Marsh, by a Classmate [W. H. H.] SCIENTIF. AMER., May 1, 1886, p. 279.

Death of Professor Marsh. YALE ALUM. WKLY, March 22, 1899.
[Tribute] by Prof. George P. Fisher. *Ibid.,* March 29, 1899.
Othniel Charles Marsh, by J. L. Wortman. SCIENCE, April 21, 1899, pp. 561–565.
Othniel Charles Marsh, LL.D., Ph.D., by H. W[oodward]. GEOL. MAG., May, 1899, pp. 237–240.
Othniel Charles Marsh, by C. E. Beecher. AMER. JOUR. SCI., June, 1899, pp. 403–428. In abridged form, AMER. GEOL., Sept., 1899, pp. 135–157; BULL. GEOL. SOC. AMERICA, vol. 11, 1899, pp. 521–537.
Othniel Charles Marsh, by H. S. Williams. JOUR. GEOL., May-June, 1899, pp. 401–404.
Professor Othniel Charles Marsh, by R. L[ydekker]. NATURE, March 30, 1899, pp. 513–514.
Othniel Charles Marsh. NAT. SCI., vol. 14, 1899, pp. 409–410.
Othniel Charles Marsh. MINES AND MINERALS, vol. 19, 1899, pp. 225–257.
Otoniele Carlo Marsh, by G. Capellini. RIV. ITAL. PAL., Bologne, vol. 5, 1899, pp. 41–43.
Othniel Charles Marsh, by C. von Voit. Sitz. k. bay. Akad., 1900, pp. 284–388.
Othniel Charles Marsh, Paleontologist, 1831-1899, by G. B. Grinnell. In LEADING AMERICAN MEN OF SCIENCE, ed. by David Starr Jordan, 1910, pp. 283-312.
The Yale Collection of Fossil Horses, by Richard S. Lull. Colls. Yale Univ., No. 1, 1913, 12 pp.
Some eminent Andover Alumni. 18, Othniel Charles Marsh, 1831-1899, by Scott H. Paradise. PHILLIPS BULL., vol. 24, Jan., 1930, pp. 16-25.
Centenary of the Birth of Othniel Charles Marsh, by Charles Schuchert and Richard S. Lull. SCIENCE, Dec. 25, 1931, pp. 647-648.

Of the contemporary newspaper accounts, the following have special interest:

NEW HAVEN REGISTER, March 20, 1899.
NEW HAVEN EVENING TELEGRAM, March 23, 1899.
NEW YORK EVENING POST, March 21, 1899 (by D. Cady Eaton).
NEW YORK TRIBUNE, April 3, 1899 (by W. E. Park).
SAVANNAH NEWS, April 9, 1899, and many other papers (a syndicated article by Ray Stannard Baker, originally intended for McClure's Magazine).

Walter Jones

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OF

WALTER (JENNINGS) JONES

1865–1935

BY

WILLIAM MANSFIELD CLARK

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

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Those who knew Walter Jones use thread of gold for the warp of the tapestry that they weave on the loom of memory. They float the golden warp for the figures of the eloquent teacher, the keen investigator and the kind friend. Because the unique personality made extraordinary impressions, every weaver uses highly colored, homespun yarn for the wefts of his tapestry.

While we may draw the figure of the scientist from the written record, we have little more than recollections with which to sketch the man. Walter Jones seems never to have thought of retreat into the realms of his own recollections. He left among his effects, as records of his career, only an incomplete set of reprints, a list thereof, and three diplomas. Indeed, this adventurous warrior burned every bridge as he left it behind. It was his habit to tear up a letter while he still mused upon the content. He wrote few extensive letters and when at long last he had a secretary he was known to go to the typist of another department that he might dodge the carbon copy that his secretary would have filed. When he retired he thrust some of his own choice possessions into the hands of assistants who had carried on faithfully during his illness.

Fate conspired in this plotting of obscurity. At every place in Walter Jones's career circumstances or accidents prevented or destroyed the written record of his ordinary activities.

Since living the moment at its full was characteristic of the man, his biographer must not complain that Walter Jones left little with which to document events or to verify reports of his ideas. And yet we wish there were a substantial body of documentary evidence concerning the thoughts that were not constrained by the merciless rules of scientific publication. It would be interesting to have enough to discover some relation between the attitude of challenge that Jones bore in daily life and the part he played in science. It might be inspiring, could we discover the aspirations of the whole man in the eloquence of his lectures. And, I suspect that there would disappear misunder-

standing of tales that have become legendary, were it possible to resolve by documentary evidence some of the enigmatical parts of this character.

I venture to suggest that a good deal of the mischief in which he gaily indulged was his delicious way of telling people not to take too seriously the intensity of his feeling in matters that he had to take seriously. Certainly, the torrent of Walter Jones's conversation carried none of the wreckage of a reformer. It was not a muddy freshet; it roared perennially in a cañon cut deep in the stratified thought of his time.

So much conjecture would have to enter serious attempts to resolve some parts of this character that a biographer had best hold himself in check. Nevertheless, it seems permissible to present certain facts of the life in suggestive juxtaposition and with such comments as will be illuminating while recognized as tinged with the biographer's suppositions.

Walter Jones's forebears were Marylanders through several generations; the paternal ancestors being dwellers on the Eastern Shore of the Chesapeake Bay country, and the maternal on the Western Shore. For finding or checking the following information about them I am indebted to Miss Harriet Perkins Marine.

The fractionation of Walter's genes through the straight male line begins with Levin Jones [b. 1799, d. 1878] the father, and thence through Levin Jones the grandfather to William Jones, a first settler from Wales. Levin Jones (the son of William) married Nancy Jones [b. 1769] who was the daughter of Roger Jones and Elizabeth, his wife. Roger Jones was an ensign and captain in the War of 1776 and his brother Thomas was a colonel who, by family tradition, was an aide to General Washington.

The most picturesque of Walter's collateral ancestors who fought in the War of Independence was Colonel John Jones (son of Colonel Thomas). One of his most famous exploits was performed when a group of three British transports with troops put into the Little Choptank seeking refuge from a storm. The story is that the Colonel summoned his neighbors who took positions behind ice banked high on the shore by the storm. From these natural ramparts at daybreak they peppered the

vessels with shot and slugs from their duck and squirrel guns. Perhaps it was "36 hours" of peppering or perhaps it was an argumentative skill, so dominant as to turn up again in great-grandnephew Walter, that finally induced the British captain to surrender when once he had lost the first point of the argument by raising the white flag.

Through his great-grandmother, Nancy Jones, Walter Jones was fifth in descent from Justice John Jones [b. 1699, d. 1774] and his first wife, Sarah Woolford [b. about 1704, m. about 1726, d. after 1775]; sixth in descent from the first settler Thomas Jones of Somerset-Dorchester Counties and his wife, Martha Davis [b. 1670, m. about 1688], daughter of William Davis, who came to Maryland from Virginia in 1664. Sarah Woolford, the daughter of Colonel Roger Woolford [b. 1670, d. 1730] and Elizabeth Ennalls [b. about 1668], descended from the following first settlers of the Province of Maryland: her grandfathers, Roger Woolford [b. in Wales, d. 1701] and Bartholomew Ennalls [d. 1688] and her great grandfather, Levin Denwood [b. 1602, d. after 1665].

Thence came to Walter's family its pride in having descended from Welsh ancestry and from early settlers of Maryland and Virginia; a pride to which Walter was indifferent.

Several of the men on the male and distaff sides of this part of the ancestral tree were holders of large lands in Maryland and Virginia and active in the services of their local governments.

Levin Jones, Walter's father, left the Eastern Shore and settled in Baltimore as a ship chandler with his business at the Light Street Wharf. He had gained the sobriquet *Captain* by way of owning a small fleet of vessels. He was a substantial business man and owned considerable property. It seems appropriate to mention his nervous habit of shuffling coins while he spoke, for Walter's similar habit of tearing letters while he mused had consequences that we regret. The father died when Walter was thirteen years of age. His widow, who was twenty-two years younger, survived him twenty-eight years.

Walter's mother, Zeanette Jane Bohen [b. 1821, m. 1840, d. 1906] was the daughter of James Bohen [b. 1797, d. 1840]

and Sarah Ann West [b. about 1800, m. 1820, d. prior to her husband]. James Bohen traced his ancestry [Bohn] to an ancient English family of noble estate. Sarah Ann West was the daughter of Benjamin [Ben] West and Annie Spencer and was related to the Wests, Spencers, Hopkinses and other prominent families of Prince George's and Anne Arundel Counties. Walter's mother, like her husband, was very active in the affairs of the Methodist Church in Baltimore. She was one of the founders of The Nursery and Child's Hospital and of The Home of the Aged of the Methodist Episcopal Church. She was one of the vice-presidents of the latter institution at the time of her death.

Walter Jones was born at Baltimore in the City's period of distress. The date was Friday, April 28, 1865. This was two weeks after the death of President Lincoln became the climax of the Civil War. That war's effect on Maryland is described by Mr. Gerald Johnson.[1]

"Driven by internal dissension, drawn by affection in one direction and by interest in another, suspected and reviled by both sides, exposed to all the horrors of war without enjoying its fierce exaltation, sharing the dangers, the losses and the woes of both North and South, but never with any part in the triumphs of either, it [*Maryland*] was trampled under the feet of both contestants and emerged beaten and broken."

Walter Jones was too original to have imitated those of his generation who ruminated what their parents had taken in, but he must have lived on the war's aftermath. Other men have said that this storm-spoiled fodder sickened them; emotionally Walter Jones had a weak stomach. In a letter concerning the World War the ageing man did not write of issues, or of defeat, or of history; he wrote of confusion. Throughout life Walter Jones frequently gave the impression of one who felt the world to be out of joint.

The Civil War also occasioned the first of the obliterations of the records pertaining to the fateful Walter. The churches at that time were lax in keeping records. Several hid their books. Diligent search therein has failed to reveal any church

[1] *The Sun Papers of Baltimore*, by Gerald W. Johnson, Frank R. Kent, H. L. Mencken and Hamilton Owens.

record of Walter's birth and baptism. Indeed, it has failed to bring to light any record concerning the birth or baptism.

Could a record be found it might illuminate a curious matter. Walter was given the middle name *Jennings* for a physician and friend of the family. *Jennings* was used on his wedding invitation and in one autobiographical sketch. The initial *J* appears on the paper he published with Stone. But it is alleged that later he dropped the middle name because he had heard that it was not pronounced at his baptism. Consequently few of his later friends ever heard of *Jennings*. On being told of the middle name one of his friends declared testily, "His name was Walter Jones!" So be it. His work clearly distinguishes him from that brilliant and eccentric barrister of the same name who won a place in the history of Virginia.

The family life that was to be Walter's lot was that of well-to-do people. Both the father and mother owned considerable property. The family life seems also to have been that of people moderately well educated according to the common standards of the time. Thence must have come naturally to the boy those simpler graces encompassed in the larger meaning of *grammar*. Schooling alone hardly could have given the mature man his unerring ease of expression, although it may have polished that handwriting which recipients of letters have likened to engravings.* It is evident that the boy was familiar with the better known classics of English literature and that his love for music had early nourishment. On the other hand, there seems to have been no close association of the family with any of those intellectual vocations or avocations that might have fixed a comfortably conventional attitude toward academic ideas. On the contrary, the ideas current in Walter's familiar environment were sufficiently undomesticated to have made him unconcernedly used to the appellative *eccentric*.

The evolution of the family has established that the youngest of a large family has the inalienable right to be mischievous. As the thirteenth child, Walter was placed, tentatively, in a very favorable position; as the last child his opportunity was assured.

* The tremulous execution of the signature under the portrait is of late date.

The temperament to seize it was determined by the genetic dice that turned up a strange group of characteristics—among them volatility. When Emerson remarked, "We boil at different degrees", it was not incumbent upon the essayist to add what his experiences as a chemist and as a moralist had taught, namely, that a boiling point is a function of restraining pressure. This principle seemed inapplicable to the spirit of Walter Jones. No occasion was so oppressed with dignity as to suppress his sense of humor; no personage so high that he would not dare to banter. In the light byplay of laboratory life Walter Jones was frequently up to something. His grandest opportunities for mischief arose from his conversational ability. He was a brilliant conversationalist and yet so forthright that he said of himself, "When I talk the loudest, I know the least". He could be frank to the verge of offense and yet he had a student say of him, "One always knew where one stood with Walter Jones and that was a great comfort". Of course, so forthright, frank and vigorous an ideologist would develop a reputation of a sort. The sort Walter Jones himself would indicate by twirling his finger about his head. Here was his grand opportunity. On occasion, this creature of opinion would be put into action to make the creators dance. On occasion, the creature was made a convenience. An undesired applicant for laboratory privileges was told, "You won't want to work here. I'm crazy."

The house where Walter was born was located on South Sharp Street, now called Hopkins Place, between Lombard and Redwood Streets.

There may be a significance more than geographical in the fact that a brother has described the particular location of the birthplace by its position relative to a Baptist church, an Episcopal church and a house "where a venerable Hicsite Quaker lived." Both parents were devout Methodists in a city rich in Roman Catholic traditions. From the fact that the mature man seldom, if ever, was tripped on a catch-question regarding the Bible, it is fair to assume that the early catechistic training was intense. Indeed, there is testimony of long Sunday school attendance and teaching. Some near relatives were addicted to strict religious observances. Both the father and the mother were promi-

nent in the affairs of their church and had the reputation of being generous to a fault in the giving of their services and their fortune to the work of the church.

In recalling this familiar background, it is important to realize that the boy was imbued with a faith which was part of a great movement, sometimes called the romantic rebellion against an earlier "age of reason". The boy was born to a generation of followers who found themselves resisting the devastation of a new age of reason. Among the many issues "Darwinism" was but one and "The Higher Criticism" one of the others. In its own peculiar way each of several issues seemed to concern something worth fighting for. The time was to come when the significance of these issues faded, either because some people had tiptoed around them or because others had risen to new enlightenment without succumbing to that evil aspect of tolerance, indifference. But let no one now, in hauteur, point merely to straight-laced practices of the stock from which Walter Jones came or attach *superficial* implications to the subject matter of Walter Jones's discussions. In all of his discussions there must have been more poignancy than this generation can feel.

There need be no hesitation in writing that Walter Jones made much of theological discussions. Whatever may have been his religious feeling at any time, his theological views were not so precious as to shield them from courteous biography. He proclaimed his views to confreres, to agents for chemical supplies, to his bank clerk, and to his friends among the clergy. Probably no one ever knew his deeper musings and probably no label would have been acceptable, but it was made plain that he could not reconcile the tenets of theology and science. From much testimony of his attitude I select one bit that seems to jibe best with the whole. It is a straightforward story told by one who listened to a long series of conversations. This was during Jones's correspondence with an eminent physicist who had publicly declared his own reconciliation. It is reported that Jones maintained the highest respect for his correspondent and earnestly looked to him for a constructive contribution. But answers came that seemed to Jones evasive and to be carrying logic into the

fog of wishful thinking. Then he gave vent to one of those tirades that too often led people to say "he baited his victim." Rather it was that Walter Jones, facing in his way the greatest problem of his age, saw the devil and threw his inkwell.

It goes without saying that tirades on religious subjects and a good deal of provocative banter is a combination likely to lead to grave misunderstanding. Therefore it is fortunate that Walter Jones worked among men whose instincts gave a natural respect to his peculiar need for freedom of thought and freedom of expression. Without this perfectly natural respect Walter Jones could not have done the scientific work that he did. This man of complete independence could not have endured freedom at the cost of condescension. There was no need. That grand gentleman, John Abel, who held the control of Walter Jones's early academic career, always kept first things first. He respected Walter Jones's scientific ability and in this atmosphere Walter Jones won his own way and fame.

As a boy Walter was active in sports. He hunted and was a very good tennis player. He is remembered by a nephew, of similar age, as "one of the best fancy ice skaters in Baltimore, the envy of the younger ones as he performed on Sumalt's ice pond." No one seems to understand why he suddenly ceased to be active out of doors, but there can be no doubt that he maintained his interest in sports. He told Professor Abel that he "went with the football crowd at Purdue" and throughout his life at Johns Hopkins he was an ardent lacrosse fan and a vigorous critic of the players not only in their contests but also in their routine practice. Some early habits may have determined the carriage of the tall, lank professor who was physically active and gracefully so.

The young man played the piano well enough to afford entertainment. The mature man collected records of classical music and knew them well. In music Baltimore offered him opportunities that he seldom missed and once mentioned to a class in this manner: "The usual recitation is scheduled for four o'clock tomorrow. Those of you who are uncivilized do not know that the Boston Symphony Orchestra is to give a special concert at that hour and so you will report here promptly. *I* shall not be here!"

Walter's early education was obtained partly in small private schools of his neighborhood and partly in public schools. Whatever these may have contributed was enlivened by the devotion of his sister Annie who followed his studies keenly and who in later life declared that she had attained a college education through Walter's eyes. This personal touch may well have exerted its potentially great effect. Tutorial habit exercised with a loving pupil can fix the photographic image. It can develop the high lights. That Walter's image of a lesson was clear is attested by college mates who heard him recite. The mature professor expected the same of his pupils. They could laugh at his extravagance while feeling the significance of his exclamation to a forgetful student, "Even the Baltimore street car conductors know the amino acids!"

In 1879 Walter entered the City College of Baltimore for its five-year course. The College has no record except that he completed the course creditably in the spring of 1884.

The following fall, Walter Jones registered in the Collegiate Department of The Johns Hopkins University, stating, "I wish to take a course which will be principally mathematics and Latin. I also wish to study chemistry, French and German." He took the courses then known as Group IV with chemistry and physics predominating in the last two years. With his progress in course his grades improved until he became a high standing student and won a University Scholarship for the year 1888-9. Granted the B. A. degree in 1888, Walter passed into graduate work in chemistry, taking minors in mineralogy and geology. Work for his dissertation was done under Professor Ira Remsen. He was granted the degree of Ph.D. in June, 1891.

Since few of the contemporary students recall anything especially worthy of note regarding Walter Jones, it may be inferred that he then exhibited what was characteristic of him in later life—supreme independence in going his own way.

On September 1, 1891, there occurred in St. Paul's Church, Ocean Grove, the marriage of Walter Jennings Jones and Grace Crary Clarke. Miss Clarke was the daughter of the Rev. and Mrs. George Clarke of Ocean Grove, New Jersey. To this seaside resort Dr. and Mrs. Jones went frequently on vacations.

Soon after his marriage, Doctor Jones took his bride to Springfield, Ohio, where he had been engaged as Acting Professor of Natural Science in Wittenberg College. The engagement there was for one year during which the professor of that subject, A. F. Linn, returned to Johns Hopkins to complete some investigations under Remsen. As was customary in those "good old days", Professor Jones "offered courses in chemistry, mineralogy, zoology, and botany. It is possible that he may have offered a course in crystallography." [2]

His first appointment terminated, Dr. and Mrs. Jones returned to Baltimore. There, August 13, 1892, was born their only child, Marion Eleanor.[3]

One may read between the lines of the following, taken from a letter concerning The American Society of Biological Chemists.

"The meeting of the Society during Christmas recess has always been a matter of regret to me, for this is the time of year that I have been accustomed to devote to home affairs since I was a child and it is very difficult indeed to break away from such a custom."

His granddaughter, Charlotte, made Walter Jones her devoted playmate.

In September, 1892, Doctor Jones went to Purdue University as Professor of Analytical Chemistry. Winthrop E. Stone, Professor of Chemistry, had been made vice-president of Purdue and already was taking over a good deal of the administrative routine for which he soon was to assume full responsibility as president. Yet, when Jones arrived, Stone drew him into an investigation on which the two men reported. Jones later contributed an article from Purdue on a problem related to a subject then lively at Hopkins. Dean Enders informs me that the records of Purdue University contain nothing regarding Walter Jones other than that here given. That Jones finally became dissatisfied at Purdue he told Professor Abel. At any rate Jones returned to Baltimore without a job and again taking up work under Remsen was made a Fellow by Courtesy for the year 1895-6.

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[2] Letter from Dean Shatzer.
[3] Marion married Gilbert A. Jarman of Baltimore.

The only enlightening information from Wittenberg and Purdue is that Walter Jones was then a vigorous teacher who used unique ways of stimulating student interest and who challenged students to questioning.

In March 1896 Professor John J. Abel took Jones to the medical school where he was appointed Assistant in Physiological Chemistry for the remainder of the academic year. There Walter Jones was to remain for the greater part of his career.

Since Walter Jones was to become the head of the Department of Physiological Chemistry in the Johns Hopkins School of Medicine, it is well to review briefly the early history of that department.

The original plan for the Medical Department [4] of The Johns Hopkins University included the teaching of chemistry to students of medicine under the auspices of the established Department of Chemistry. Indeed, the first *Announcement* lists the head of that department, Ira Remsen, as Professor of Chemistry in the roster of the Medical Faculty. Remsen held this title until he became the second president of the university in 1901. Remsen had been one of the committee to consider the establishment of the Medical Department and he continued on its Advisory Board, first as an elected member and finally *ex officio* as President.

With the opening of the school in 1893 the original plan of teaching preclinical subjects was modified as the exigencies demanded. The teaching of physiological chemistry was entrusted to the professional school under an arrangement stated as follows in the first *Announcement* of 1893. "The instruction in Physiological Chemistry will be for the present under the charge of Dr. John J. Abel, Professor of Pharmacology, with the aid of an assistant."

It is important to note that physiological chemistry was given a unique position in the new school. The original intention to have medical students trained under the guidance of the Depart-

[4] The name was changed during President Goodnow's administration to harmonize with *School of Hygiene and Public Health, School of Engineering,* etc., and is now *The School of Medicine, The Johns Hopkins University.*

ment of Chemistry was a reflection of the purpose to train medical students as university students. The actual, initial placement of physiological chemistry in the Department of Pharmacology broke an historical relation that had made the subject the foster child of physiology. The entrusting of the teaching to Professor Abel gave an especially noteworthy character to the whole affair.

Professor Abel had come, directly and indirectly, under the influence of several German investigators, who, although trained in the general field of medicine, had acquired an expert's knowledge of chemistry and who devoted it to fundamental work. This influence, and Professor Abel's own keen appreciation of chemistry, made him determined to give the science its full due. He wished to avoid the limitations of the purely "analytic school" and the subordinate place of chemistry suggested by the term "chemical physiology." Abel's attitude is reflected by Jones's specification for an assistant that he sought in 1925, nearly thirty years later.

"We want a man who has a Ph.D. degree, who has teaching ability as well as research ability and who is well grounded in the fundamentals of chemistry. Of course, it is desirable that he should also have had a training in physiological chemistry and the biological sciences but he must be a chemist primarily."

While the burdens of organization and the financial catastrophe in the early years of the school inhibited the development of Abel's plans, he managed to create the plant out of which brilliant investigations were to blossom. It was at the beginning of this flowering that Walter Jones came.

When the school was opened in the autumn of 1893 the students, of course, had not reached pharmacology and Professor Abel, with his first assistant, Dr. Thomas B. Aldrich, could handle the introductory course in chemistry. Thereafter, Abel turned over the instruction in this subject to assistants. There was also a spatial separation of the work, chemistry being left for a time in the old Pathological Building and pharmacology going with anatomy to the newly built (1894) Women's Fund Memorial Building until chemistry and pharmacology were reassembled in the Physiology Building in 1898.

Thus, when Jones came to assist in the teaching of physiological chemistry, there was no autonomous department for that subject, although the subject was taught as a distinct discipline. At the same time the close relation between subjects taught under Abel's general supervision carried Jones into the teaching of toxicology.

The list of Jones's successive titles is instructive:

1896-8 Assistant in Physiological Chemistry.
1898-9 Assistant in Physiological Chemistry and Toxicology.
1899-1902 Associate in Physiological Chemistry and Toxicology.
1902-8 Associate Professor of Physiological Chemistry and Toxicology.

1908 (Department of Physiological Chemistry established).
1908-1923 Professor of Physiological Chemistry.
1923-1927 DeLamar Professor of Physiological Chemistry.

When Jones was made full professor in 1908, the Department of Physiological Chemistry was created. The new title, conferred on Professor Jones in 1923, commemorates the School's benefactor Captain DeLamar. The choice of the Professorship of Physiological Chemistry to receive his name, as one of several means of commemoration, gives recognition to Captain DeLamar's acquaintance with one branch of chemistry and to the specific part of his will that refers to his interest in nutrition.

The order of arrival of those who assisted Professor Abel and were officially designated as Assistants in Physiological Chemistry were Thomas B. Aldrich (1893-1899), Edwin S. Faust (1895-6), Walter Jones (1896-1908) and Arthur S. Loevenhart (1905-1908.[5]) During parts of this period Abel had but one assistant assigned to physiological chemistry. Indeed, during most of his term, Jones alone handled the teaching of chemistry. When Jones became a professor he had but one assistant most of the time up to the end of the World War. They were:

Arthur H. Koelker (1908-11)
Eli Kennerly Marshall, Jr. (1911-14)

[5] After 1908 Loevenhart stayed on for a time in the Department of Pharmacology.

D. Wright Wilson (1914-1922)
Annabella E. Richards (substituting for Wilson 1918-19 during his leave on military service)

After the World War there came
Mary Van Rensselaer Buell (1921-1930)
Marie E. Perkins (1921—)
Lawrence Wesson (1922-25)
William Hoffman (1922-27)
Herbert O. Calvery (1925-27)

This roster does not include an occasional assistant not listed in Jones's own prospectus of the course.

The list is of interest itself, and also because its comparison with Jones's bibliography proves that, prior to the last period of his work, members of his staff seldom entered his field of research. Indeed he advised them not to do so.

After Doctor Jones had been with him a few years, Professor Abel advised him to study under Kossel. This advice was accepted but a financial difficulty stood in the way. Indeed there were times in Walter Jones's life when he had to be so absorbed in saving money that he feared he was becoming a "penny pincher". He related that when this fear was felt he had great satisfaction in throwing a penny into Jones Falls [6] as he crossed the bridge on his way to work. Appreciating the financial difficulty, Professor Abel had an inspiration one day when a sheriff appeared with the organs of a woman suspected of having been poisoned. While Abel was loath to have his laboratory burdened with such cases, he arranged to have Jones take this case. When Jones had identified strychnine he went to the distant town with his evidence. He found the people in an uproar over some scandal and a trial impracticable to hold, so he returned disappointed. At last he testified and brought home the needed fee.

The visit to Germany was short, June to December, 1899; but within that period Jones accumulated the data for two papers and became so inspired by Kossel that he devoted himself thereafter exclusively to the study of nucleic acids.

[6] The stream that runs through Baltimore.

It was Dr. P. A. Levene, the other American leader in this field, who welcomed him as Doctor Levene himself tells in the following letter.

"My memory pictures the arrival one day of a lean, tallish American of rather indefinite age, somewhat forlorn in a foreign land which he was visiting for the first time and, as I soon found out, having with him a wife and child. As my own sojourn in the city of Marburg had already had a history of a month or six weeks, I considered myself called upon to help Jones and family to find living quarters which was not a very difficult matter since the lady who sheltered an Englishman and myself, both of us working in Kossel's laboratory, was only too glad to make room for an American who could afford to bring a family with him all the way across the ocean and who, in her imagination, must be one of the American millionaires.

"The events that follow are rather vague in my memory and I am certain the fault is mine for Jones was not a man to permit a day to pass without leaving some impression on it. However, it so happens that besides Jones, two Englishmen, and one Italian, there were in the laboratory three Russians, a Frenchman, and myself and somehow, the Franco-Russian alliance is more vivid in my memory than that of the rest of the 'Internationale.'

"I remember clearly that as soon as Jones discovered that English was the dominating tongue of the Internationale—the Professor speaking English and the Russians being silent in every language but their own—he dropped his shyness and was ready for argument. Great enthusiasm and force of expression revealed themselves soon and, before long, I became a victim of them.

"Jones and myself shared a long laboratory bench but not too long to prevent occasional discussions. Jones was given the concrete problem of making derivatives of thymin. The work was progressing successfully and was destined to shape his principal interest in nucleic acids. My own problem was rather fantastic for I conceived the idea that vitellin must contain the chemical nucleus of nucleic acids since the nucleins developed during the growth of the embryo in the yolk. The problem being fantastic, and originating with myself, it naturally progressed poorly. Kossel was of much help to Jones and little to me. Jones became a devoted admirer of Kossel. I was more impressed by Hofmeister who, an enthusiast of the type of Jones, was certain that he held the key to the solution of the structure of the protein molecule. Jones was an arguer by nature; argument is the Russian sport par excellence. We had a lively argument and I was floored in the first though protracted bout. To save my ego, I blamed my defeat on the fact that Jones had the better command of English.

"So the friendship of Jones and myself began with an argument and continued in the same way for many years for we were warm friends regardless of our temporary scientific disagreements."

In the year of Jones's pilgrimage (1899) began the long series of papers on nucleic acid chemistry that constitute far the greater part of his contributions to science. These will be reviewed later.

Here we may dwell upon the sociology of his scientific research and teaching because it throws the light of one, somewhat isolated, case upon an important era of American physiological chemistry.

The views that Walter Jones held with respect to his dual rôle as an investigator and as a teacher, the adjustments that he made in relating a rapidly growing science to pedagogical problems, indeed, his whole attitude toward his position, present an enigma. Testimony conflicts and conflicting with that testimony which would make of Walter Jones a man interested only in nucleic acids stands one remarkable fact: the few letters that have come to light contain little of interest in regard to nucleic acids or similar scientific matters and much that bears upon his pedagogical problems. There is too little of this for one to dare a reconstruction of Walter Jones's views. If this sole body of documentary evidence is to be used, quotations must be placed against the background of his scientific career.

We may mention first the theoretical attitude toward chemistry in the new school. Perhaps there is no better way to do so than by reference to the addresses of Professor Welch,[7] for, while Welch may have appropriated many of the ideas that he expressed, no one preserved better their perspective in the general scheme and the very fact of some appropriation made him recognized as a spokesman.

Considering the time (1894) at which he spoke, Welch displayed amazing perspicacity in the following remark.

"Physiological chemistry means much more than what is usually taught in our medical schools as medical chemistry, which includes little more than the chemical analysis of certain fluids of the body for diagnostic purposes."

[7] William Henry Welch—Papers and Addresses, Vol. III.

Continuing, Welch put the seal of his approval on the following quotation from Hoppe-Seyler.

"I cannot understand how in the present day a physician can recognize, follow in their course, and suitably treat diseases of the stomach and alimentary tract, of the blood, liver, kidneys, and urinary passages, and the different forms of poisoning, how he can suitably regulate the diet in these and in constitutional diseases, without knowledge of the methods of physiological chemistry and of its decisions on questions offering themselves for solution and without practical training in their application."

These quotations express in fairly definite terms the attitude that led the authorities to give a carefully considered place to chemistry in the medical curriculum.

There was also considered the important factor of the student's preliminary, liberal education of which Welch wrote that ". . . it should not be taken with reference to any utilitarian purpose. . . . when studying chemistry it should be done with the object of learning the general principles."

In general the effort was made to keep the Medical Department more than nominally a part of the University; ". . . ideals of the university must inspire the whole life and activities of the medical department." These ideals were to permeate the clinical departments where medicine should be treated as "one of the natural sciences".

All this had a great deal to do with the relation of Walter Jones to his students. It set up a guiding syllogism the first parts of which were made visible in the School's catalogue.

1. "The medical art should rest upon a suitable preliminary education and upon a thorough training in the underlying medical sciences."

2. Chemistry was made an important member of the "underlying medical sciences" by imposing an unusual entrance requirement, and by making physiological chemistry a prerequisite to the study of clinical subjects.

The third part of the syllogism was not stated in the catalogue but there was clearly the implication that a body of thought, the initiation of which had been so carefully planned, would be

carried on to the final objective. Welch stated this in 1899 when, after having previously indicated the advantages of keeping the pre-clinical subjects basic and free from too many clinical applications, he said:

> "The real aim of medical education should be the training of practitioners of medicine and surgery, and the benefits of thorough grounding in the fundamental medical sciences are to a large extent sacrificed if the student does not find in the latter two years of his undergraduate study well-conducted clinical courses which afford opportunity for the practical application of knowledge previously acquired in the laboratories."[8]

Whether Walter Jones thought out the position of his department in this educational scheme or merely slid into the policy that he followed is immaterial when set beside the fact that he did seek to train students in what he considered the fundamentals and did leave clinical applications to be taught where used. In principle, this was in harmony with the general scheme. In fact he applied, however imperfectly, that part of the declared syllogism for which he was responsible. Could it be completed? The answer depended upon an appreciation of events.

Welch realized that physiological chemistry had replaced what he called "medical chemistry." If we continue to discriminate, as Welch did to emphasize developments, we shall find that during Jones's career there developed need of further distinction within chemistry. While the older physiological chemistry arising in medical centers had played with the fringes of clinical subjects, its more substantial new contributions were bringing the basic science to those developments of its biological phases that became the foundation of *several* branches. Among these was *clinical chemistry*. Furthermore, as the several parts of biochemistry's application carried topic after topic out of the hands of the medical profession and into the hands of special groups, the evolving clinical chemistry became, theoretically, the particular concern of the medical profession.

Within the period of Jones's career new analytical tools had made possible sufficient knowledge of body components to provide several new categories of thought associating material

[8] Welch: Papers and Addresses. Vol. III, p. 68.

changes with what is observed in disease. General principles of physical chemistry attained the power to deal with the interplay of multiple components as disease shifts equilibrium states throughout the body. There accumulated vast arrays of facts requiring sifting by clinical experience and, where possible, reduction to order by the logic of the basic science. Withal, a new perspective developed; sometimes the special science had to take courses of its own but perhaps as often it bore out the remark of Whitehead. "The paradox is now fully established that the utmost abstractions are the true weapons with which to control our thought of concrete fact." In all events, the broader knowledge had given scope to a type of thought known of old to clinicians but requiring discipline in new bodies of fact and logic. What Welch had decried as "medical chemistry" should have been relegated to special technical laboratories. Physiological chemistry remained essential, but, speaking broadly, it had expanded as a general subject, very unevenly and without any particular objective. Clinical chemistry was becoming a distinctive body of logic with attendant bodies of fact, not only centered on one objective but drawing its power from the broad concepts of the basic science and from techniques derived therefrom.

How much of this did Jones have in mind when, writing of the attitude of his department, he exclaimed, "Chemistry is looked upon here as the key to modern medicine."? It is reported that on occasion Jones would carry a topic from its academic beginnings to its decisive place in modern practice and that he was not above extravagant praise of the practical accomplishments of his science. This occasional jangling of the keys may have caught the attention of some students but such attractions were not needed by others. Jones made his science fascinating and it is testified that many a student was surprised to find the text so dull when the lecture had been so interesting. Here was stuff for educators to conjure with.

But the conjuring would remain lost motion so long as the new science retained only in theory the position envisaged by Welch and Abel and Jones. A student still could ask: Is a knowledge of chemistry *vital* to medical practice? It seemed a sufficient

answer that practitioners who declared their admiration of advances in chemistry forthwith would express regret at not having kept up.

Thus, Walter Jones was left to deal as best he could with that academic interest in chemistry which felt little pressure from the profession. He was led to write, "The younger men here usually take their chemistry as a perfunctory part of their medical education." As late as 1923 he complained, "Physiological Chemists are much more difficult to secure than either pharmacologists or pathologists for the reason that most students of physiological chemistry are headed toward medicine and do not stop in the former science long enough to become expert."

Had Jones taken more graduate students he could have counted on an established seriousness toward *general* biochemistry. But it would have been too much to have expected him to do so. He had too much to do almost single handed. Nor would this support of general biochemistry have met ultimately the basic cause of his complaint.

It is true that during many years the catalogue announced elementary and advanced courses in Physiological Chemistry for graduate students; but throughout the Medical School the intent of all "graduate" courses was to provide advanced training for physicians. It is not clear that any physician took Jones's offered graduate course. It is certain that only on very rare occasions was a graduate student accepted. The graduate students in West Baltimore felt that they were not wanted in the laboratory in East Baltimore and there are letters informing applicants that the department offered no training leading to advanced degrees.

Replying to Professor Henry Harper's inquiry of what he was doing for graduate students, Jones wrote in 1922: "Physiological chemistry . . . is treated exclusively as a medical subject and the entire resources of the Department are devoted to medicine. No attempt is made to train men for philosophical degrees." Thus Walter Jones was completely committed to the first parts of the syllogism framed for the training of medical students.

But also he had little inclination to be concerned with the concluding part. He had entered the school as an organic chemist with no particular interest in medical affairs, without specific

training in the evolving science of physiological chemistry, without recorded training even in biology, and he was valued initially and throughout his career as an investigator. Brutally honest with respect to his interests he nevertheless felt at home in the medical school not only because his teaching fitted the declared scheme but also because of one of the outstanding characteristics of university life at that period.

Americans had taken from an imported system only one of its better parts. This they had converted to an enthusiasm for research so furious as to have swept aside other parts of scholarship. The salutary effect of the transition from didactic teaching and from book learning to direct demonstration and observation in the laboratory needs no review. It was gloriously great and Jones gloried in being a part of it as an investigator in a new field. On the other hand the trend was so impetuous as to have obscured the simple fact that, by no stretch of the imagination, can any student reconstruct for himself a small part of what the experiments of others, now and in past ages, can give him as his scholarly heritage. In its new freedom American scientific scholarship retained no certainty of how to make research subserve the whole of scholarship. As Welch warned in 1888, the encouragement of research is not the primary conception of the true university ideal. Possessed of a native genius for organization Americans elaborated their organization of research and gave little heed to the consolidation and reorganization of the knowledge acquired. Occasionally pedagogy went wild in the lower schools for lack of adequate organization by the higher schools of the *knowledge* deserving emphasis and logical placement in the progression of training. In the higher schools datum piled on datum and theory went the way of the convenience of specialists.

Jones was distinctly a specialist and was honored as such. He also took infinite care to consolidate in his monograph the knowledge of his field and he took great pride in having done so.

For lack of consolidation the attainments of research in the field of *clinical chemistry* went as isolated contributions, not as parts of a whole; as new specializations in an over-specialized culture, not as a group of evidences from which could be drawn

principles that permeate. The methods of chemistry would be conceded their uses in medicine, but clinical chemistry, then unrecognized as a distinctive body of thought, would have to await leaders strong enough to overcome professional inertia.

Those who knew Walter Jones, the general situation, and the unique position given to chemistry by the designers of the School will feel the power of exceptionally restrained expression in the following note with which he returned a widely discussed report on the teaching of clinical subjects in America.

"I supposed, at first glance, that I would receive some information from the report on the teaching of physiological chemistry, but it deals with the subject only in so far as it is submerged in physiology."

Also there was accumulating too much to teach and there remained too little time in which to teach even a part of it. American biochemists were becoming a little noisy in their demands for longer introductory courses. Professor Jones could not be drawn into this. Not only did he accept (perhaps passively) the plan for shortened courses in all departments agreed upon at Hopkins shortly before his retirement; he also wrote sometime before as follows:

"I should say that the time devoted in the medical curriculum to physiological chemistry depends to a considerable extent upon what portion of this subject is taken up in other departments on the borderline. I think the tendency has been to teach clinical medicine in departments of physiological chemistry and this greatly lengthens the time allotted to the subject." [9]

The curious position in which Walter Jones found himself should now be evident. The vigor of his research fitted the temper of the time. Its quality brought encouragement, promotion and fame. Valued primarily for this he was given a pedagogical problem of the first magnitude involving the cultivation of such an appreciation of a developing science as would subserve professional ends that were in view but that were only vaguely recognized within the profession. Doubtless he was given this task with full recognition of his genius as a teacher.

[9] Letter of February 16, 1921.

Yet to the historian of his time it could appear that students were then regarded as needing only exposure to original minds. The year was divided so as to give Jones ample opportunity for original research. But during the teaching period he handled large classes, at first single handed, then with one assistant. There is no evidence that he felt an incongruity between this and his complaint to Dr. Abraham Flexner of The General Education Board that medical students seem to have been ill prepared in college because they had "been crowded into large classes where they are expected to work under the poorest conditions. . . . They give excellent evidence of never having received the individual instruction which is so necessary in the . . . education of most men." It was set in the scheme of affairs that he should teach principles, not practices. There was provided no systematic way to build practice on the principles that he taught. He and many others were constructing the parts of a science that were to remain more or less unarticulated. The man himself had two major academic interests—nucleic acid chemistry and good lectures. He was somewhat indifferent to the rest. Still more indifferent to anything but research had been several of his preceptors and were several of his colleagues.

In summary it may be said that Walter Jones was not a student of medical education; he was an inspiring teacher. He was not a propagandist of an evolving science; he was a creator of one of its parts.

The conflicting opinions on the apportionment of his thought and effort to each phase may arise from the asking of questions that seem inappropriate when once the setting of his work is appreciated. Thus, it has been asked whether Walter Jones should not have created a school of physiological chemistry when the period of his career was favorable thereto. Since one yard stick used to measure a teacher is the stature of his pupils in his field this appears to be a fair question and a negative answer a detraction. But let us examine the facts.

If the reader cares to study the list of coauthors in Jones's bibliography he will find the following. The list includes the names of several members of the clinical departments. In the early years of the School the "preclinical" laboratories were cen-

ters of a good deal of research by members of the clinical staffs. Later, the introduction of the "full-time" system in clinical departments was accompanied by a more systematic organization of research in those departments and the participation of clinicians in the types of investigation that were influenced by the points of view in the basic sciences measurably declined. While the going was good Jones cooperated.

The greater part of the list of coauthors is made up of medical students. The list of these medical students, some of whom Jones literally snatched from the bench of the introductory course to ensconce in his own laboratory, proves him to have been a good picker and sympathetic with the School's ideal of association between teacher and student in the rôle of coseekers. Of the fourteen medical students and four graduate students who published with Jones, nine attained academic distinction by professorial rank. One became a Nobel prizeman. It is not intended to say that Jones was mainly responsible for the training of these individuals. For example, among the medical students Whipple and Winternitz owe far more to Welch. It is intended to say only that Jones selected wisely and contributed much to the training of those who made this remarkable record.

On the other hand when we look at the careers of the *medical* students who published with Jones we find that none is distinctly in the field of clinical chemistry. To be sure, several of those who worked with Jones as members of his staff, as visitors, or as graduate students and several of the medical students who took his elementary course carried on with distinction in one or another field of chemistry or with the tools thereof. What is now under discussion is the influence of close association with medical students as judged *only* by the record of joint publication. An examination of the circumstances already cited will not detract seriously from the esteem of Walter Jones as a teacher; it will reveal the weakness of an evolving system.

Under the circumstances the trend of Jones's intimate students is not difficult to see. They were well initiated by the introductory course in which Jones hewed to the line of the established syllogism. Then Jones gave them an invaluable introduction to the serious investigative method. Finally, they came to the

clinical departments. There clinical chemistry was merely a valued adjunct, not given a position commensurate with that of morphological sciences. There established sciences had their scholars. Within their fields both the universal experimental method and the knowledge culled from the trials of ages were united in one objective. Few students could have resisted this. None deviated from the organized path to seek training by paths not organized. Such, perhaps, is one perverse manifestation of the American genius for organization.

The testimony is almost universal that Walter Jones was a brilliant lecturer. It is not equally well appreciated that he took great pains to prepare the materials of his lectures and that he did not spare himself in developing their eloquent logic. In the closing paragraph of a letter to be quoted later Doctor Read writes of those moments before a lecture in which a great emotional strain was evident. Before that, however, there had been a long period at his favorite place before the balance where he worked out the flow sheet of what was to appear as a smoothly developed subject. Perhaps they were his eloquence, his gossipy reviews of controversies, his sharp wit that brought the advanced students back to the first-year class room; but it may be doubted that these alone induced them to come year after year. There was an intellectual interest. Robert P. Kennedy writes:

> "Walter Jones's lectures, both in class room and in private conversation, were virile and impressive. No matter what sort of tirade his dissertation may have sounded like, his thoughts were logical, his expressions extremely accurate and if the listener were inclined to argue he was always worsted by a better piece of argumentative effort."

Professor Wright Wilson, while an assistant to Jones, noted the frequency of Jones's new approaches to old subjects and the dramatic quality of the lectures which were "made so interesting that students of upper classes often returned to hear him speak."

The vividness of certain impressions created during those lectures is illustrated, perhaps extravagantly, by the effect of his comparison of combustion in a mouse and a candle. A mouse and a candle were brought near to extinction under bell jars

and then revived in air—all in pantomime. A student today swears that the experiment was real! "And the wonderful thing is this", said Jones, "one has to light the candle but one doesn't have to light the mouse."

There were times in the early years when descriptive material loomed rather large in the course. Physiological chemistry was then largely descriptive. There were times in the later years when nucleic acids loomed rather large in the perspective. Emphasis here had the advantage of giving the students an insight into a subject in the making as presented by a maker and what they lost in general perspective was compensated, as many of them have attested. When others complain of such restrictions let them not forget the times.

In view of Walter Jones's training, his preoccupation with teaching and with highly specialized research, it is not surprising that his technical knowledge of the evolving applications of physical chemistry was limited. He was blatantly honest in all such matters. But I can testify that in the later years he had an appreciation of its developing importance. The following letter to Doctor Holt reveals both sympathy with one development and a characteristic regard for his responsibility to students headed toward the clinical uses of chemical thought.

"December 12, 1923.

"Dear Doctor Holt:

"After thinking over the matter that has been a subject of conversation between us and talking it over with a number of people I have come to the conclusion that the physical chemistry of proteins, as it now stands, is rather too special a matter for our second year students. Of course, an optional group of men can be gotten together for any course, as students will take your word if you say that the subject treated is of importance in medicine.

"I see no reason, however, why you should not give this optional course on your own accord and in connection with the Department of Pediatrics. I am in sympathy with it and would encourage it in every way except as concerns matters above stated.

"Very sincerely yours,

"Walter Jones."

The usual testimony is that Professor Jones was a stimulating teacher and not a soft one. One student will not forget the moment he poured the materials of an experiment down the sink, explaining, "It didn't work the way the book said." The Professor exploded, "My God, man, if you threw a brick out of the window and it went up instead of the way the books say, wouldn't you stick your head out?"

Occasionally, Jones was accused of being a bit too severe with stupid students. Another aspect is revealed in a letter regarding a candidate for an advanced degree who was subjected to an oral examination by Professor Jones. After stating that he was not satisfied with the candidate's knowledge, Jones added:

"The matter is a little annoying to me and I would not make a report of this kind if there was the slightest possibility of making any other. I suggest in Mr. ———'s interest that you substitute me on the Committee by someone else and see if a disagreement among authorities cannot be produced."

Jones could be mischievously enigmatic. To Doctor Mendel, Editor of the *Journal of Biological Chemistry,* he wrote, when forwarding a paper:

"If you will read the last page of the long article you will concede the desirability of concealing not only its contents but even the fact that I am publishing any article at all."

When admitting a clearly explained mistake of his own that Wilson had noted, Jones bubbled over with an old jibe.

"Dear Doctor Wilson:

"If you have as much difficulty in calculating in dollars and cents as you have in reckoning normal solutions, I should think you would get along pretty well in dollars and cents. Your calculation is exactly right. I now see the reason. I made arithmetical mistakes all the way through. So everything along the line is now beautiful."

It is difficult to convey the nuances of Walter Jones's acts and utterances. He would press to the hilt the thrust of his argument. If the victim felt wounded he had still to discover that Jones's glee in the game made him unconscious of a personal aspect and in time he would discover that Walter Jones could

melt into the very essence of thoughtful kindness. Again it might be thought a typical Americanism when he bullied a student into a position where he could confer on him a great favor, or it might be thought sweetness of a soft kind when he dried the tears of a secretary whom someone had offended unjustly. Not so; with many an act or word of kindness would go some unique remark that devastated the occasion for the kindness shown. Many a witticism drags in the telling for the simple reason that few can reproduce the relations of circumstance and effect on the listener. Frequently, it was only when blood oozed that the listener realized there had been a rapier thrust.

The following dialogue is reported by Doctor Rowntree to have taken place when Osler returned to Baltimore and Rowntree led him to Jones's laboratory.

Osler Well, Doctor Jones, so you are still wasting your time playing with test tubes. One of these days the Grim Reaper will come and afterward people will say: "Didn't Walter Jones do this?" Later they will say: "Didn't Jones do this?" Finally they won't know who did this.

Jones I see you've become a pessimist since you left Hopkins. No, Doctor Osler, we are not forgotten. Only the other day someone asked me: "Didn't Hippocrates do so and so?"

Osler Hippocrates! He was one in a hundred million.

Jones Oh, well, there are others. Every week some one says, "Galen did that!"

Osler Ah, but Galen was one in a hundred million!

Jones By the way, Doctor Osler, how long is it since you left Baltimore?

Osler Five years.

Jones I'll swear I heard your name mentioned in those five years.

Walter Jones's early training developed an intense idealism; the later training turned its values to the service of a "tough-minded" realism. The incompatibles encountered on the way gave his conversational abilities their chief opportunity, but they seem to have had a very deep significance for the man himself. Unfortunately, we can judge this only from the impression of a profound intellectual struggle. It is hard for me to believe that

the constant recurrence of all sorts of profound questions in his conversations meant only the creation of an opportunity to talk, as some would have us believe. Nor can I see much in individual citations of the way topics were handled. In their isolation they often lack point. It is the impression of a constant hammering that seems to denote perpetual struggle. What would appear on the surface and without warning would be something of the following order. A colleague, bumped into while passing a doorway, is confronted.

Jones, opening and closing a jack-knife, "Tell me. What do you see me doing?" A facetious reply is stopped half-said. "Answer me! What do you see me doing?"

A. "Why, Doctor Jones, you opened the knife; then you closed it."

J. "Of course, and now tell me this. When I opened the knife, what became of the closedness? When I closed the knife, what became of the openness?"

A. "But, Doctor Jones, that's . . ."

J. "Of course it is; but that's Kantian philosophy!"

No one would claim that Walter Jones was a profound thinker. Everyone will admit that he hit with deadly aim the nonsense current in the common ideas of his time. They might be ideas of primordial creation or the square root of minus one that were the topics of discussion around the lunch table. It was not his object to expound but to take the ideas that were current and lash the nonsense unmercifully. To find the significance of the fact that the square root of minus one irked Walter Jones one must not think of him dealing with that quantity's logical arrival, its place in a broad theory of numbers, or its practical use, as in electrical theory. One must recall the verbal logic without attachment of extended meaning fed to most of us in the texts of our youth—that verbal nonsense which still permeates much elementary instruction today and bespeaks the failure of the universities to organize advanced learning in such a way as to make its reduced elements senseful. Of like significance is the story of Jones's argument with a physicist over the meaning of "dextro" and "laevo" with reference to the rotation of the plane of plane-polarized light. Since few advanced treatises record the historical change of convention and hardly any text states

any convention clearly, it may have been natural that one of the disputants followed the older and one the newer convention. Walter Jones clinched his argument by rushing to his polariscope with a solution of D(d) glucose [d(+) glucose] and pointing to the direction in which he rotated the analyzer.

So, in all matters, Walter Jones demanded attachment of significance to words,—where possible, significance that could be made concrete and demonstrable. There at last he found his joyful satisfaction and his willingness to forego the delusive satisfactions of the philosophic WHY. Said he, "No question in biology that begins with *why* will ever be answered."

Yet he never questioned the satisfactions of others. Because his friends in all callings felt this, they would allow Walter Jones to hit his hardest. And hit he did to the consternation and discomfort of the unknowing.

In the fall of 1923, Doctor Jones wrote of "a remarkable weariness in my left leg after walking a few blocks." It soon became evident that he was suffering from thrombo-arteritis and in January he was unable to meet his classes after the first lecture. Thereafter he appeared at the laboratory only to discuss general matters. The unaccustomed restraint worried him no end and finally he admitted, "I have a case of 'nerves' ". Then he abandoned work entirely. He retired in 1927 and recovered his health slowly and incompletely.

In 1921 tentative plans had been made for "a new building for physiological chemistry". The immediate necessity arose from the expanding work of the Departments of Physiology, Pharmacology and Physiological Chemistry and of the offices of administration, all housed in the Physiology Building. Chemistry was confined to the attic. The state of affairs there may be imagined from the fact that the hood available for students was so badly ventilated that adjustments of Kjeldahl digestions had to be made between rushing into and out of the room with bated breath. The purveyor of student supplies could hardly turn around in his coop. Every inch of space, including that under the eaves, was used, and two polariscopes were set up in a room so small that the doorway was in constant use as a means of turning around.

Therefore, it should have been with high hopes that Professor Jones accepted the chairmanship of a committee on plans for a new building. Nothing concrete was accomplished until after Jones's retirement. Then, after the completion of the "New Physiology Building" in 1929 there occurred a little incident that should be recorded.

After a long absence from his old department, Walter Jones appeared to lunch with the new staff. As of old he held the course of conversation, and long enough for a secret messenger to obtain flowers for the laboratory that had been reserved for the Professor Emeritus. Then he was led to his new quarters. There Walter Jones became for a moment his old self sending the faithful "Andy" on trips for glassware and reagents. Suddenly, he disappeared never to return. Later it was learned that there had come over him a discouraging sense of ground lost and of lack of energy where energy had known no bounds. Nor could he be induced to try again where once the sense of commanding superiority had been lost. He knew then that he had retired completely to pleasant places on the shore or in the mountains during summer months and that music and bridge must keep him diverted in the winter. For excitement, he played with the stock market or drove his car "two thousand miles a month" searching all the byways of Maryland's beautiful countryside.

Walter Jones died in Baltimore, February 28, 1935. Mrs. Jones died the following year, November 18, 1936.

Before we take up the record of his scientific work, let us revert to the happier days before the incapacitating illness as those days are described in the following letter from Dr. B. E. Read.

"Henry Lester Institute of Medical Research
"Shanghai, China
"May 25, '35

"Dear Professor Clark:

"I regret that foreign travel has kept me from storing old letters and that I have not preserved any of Walter's delightful compositions.

"It was upon Doctor Welch's recommendation that Professor Jones was willing to accept my unworthy self to work in his

laboratory in 1916 and 1917 for eighteen months' intensive study of nucleic acids and derivatives. I was privileged to enjoy the warmth of his friendship which for years afterwards he sustained by an occasional postcard or message through mutual friends.

"Jones's appreciation published in Science [10] is an admirable summary of his rare qualities. I, for one, realized quite frequently that his warm personality could be scorching hot. In his contacts with his fellow men his mind acted as a refiner's furnace, from which the more timid shrank, by which the unaware were rudely surprised, and in which the more courageous found a degree of salvation.

"He had an intense love of his fellow men unappreciated by the victims of his assaults,—assaults upon everything small, weak or mean in men around him. At the conclusion of his speech at the New York dinner of the Federation in 1917 he confessed his own creed in the words:

" 'This above all: to thine own self be true,
And it must follow, as the night the day,
Thou canst not then be false to any man!'

"Walter Jones's contemporaries will witness to the high order of his intellect, thousands know of the brilliance of his classroom talks, a few, like myself, were privileged in a remarkable way to know his daily round of sound reason and extravagant wit. Whilst Walter Jones isolated himself from his fellows more than any scientific man I have known, intellectually he saw them and himself as one in the great struggle for the triumph of reason; and underneath his mocking laughter was a human sympathy of a very fine order.

"I had more courage than usual one day when I said to him, 'Professor Jones, I saw an unusual example of American spelling this morning.'

" 'Oh, what was it?'

" 'A negro fish shop on Monument Street had a notice of "CLAMPS" for sale.'

"Jones's reply was instantaneous, 'Oh, that's nothing. When I was in England they gave me lead in my plum pudding.'

"That kind of banter made guanylic acid not quite so sticky and the morning's work passed quickly and Wright Wilson, Sam Goldschmidt and Eddie Plass would gather round the ham sandwiches for a session of seasoned wit more carefully considered and narrated.

[10] Science, March 29, 1935.

"Before his brilliant lectures Jones would pace his private laboratory in agony like a mother praying for the life of her child. More intimate details would reveal the fact that in his synthesis of thought and feeling Walter Jones exhibited real genius.

"Sincerely yours,
"Bernard E. Read."

AFFILIATIONS WITH LEARNED SOCIETIES

The National Academy of Sciences. Elected member in 1918.
The American Physiological Society. Elected member in 1898.
The American Society of Biological Chemists.
Councilor in first group of officers, 1907, and in 1920 and 1921.
Treasurer 1910-12.
President 1915 and 1916.
Journal of Biological Chemistry: Editorial Committee, 1905 (vol. 1) to 1929 (vol. 84).
Society for Experimental Biology and Medicine.
Elected member 1905. Resigned 1921.
American Association for the Advancement of Science.
Elected member 1928, Fellow 1931.
Phi Beta Kappa. Elected member 1906.
Sigma Xi. Signed the petition for the establishment of the Johns Hopkins Chapter. No record of membership.

SCIENTIFIC WORK

Walter Jones's dissertation (1891)[11] describes the preparation and analyses of some new sulphonphthaleins. The work was part of a series of investigations being carried out in Remsen's laboratory and is of little importance beyond that extension of the record which is necessary in such cases.

As an associate of Winthrop E. Stone at Purdue, Jones was co-author of a paper (1893) regarding the digestibility of pentosans. In 1894 he published from Purdue a note regarding the dichlorides of orthosulphobenzoic acid. Others had reported that the reaction of phthalyl chloride with "hydrosulphide" yielded

[11] Published in the *American Chemical Journal* (1895).

$C_6H_4 \begin{array}{c} CS \\ <\!> \\ C{=}O \end{array} O$. Jones expected a similar compound on similar treatment of a dichloride of orthosulphobenzoic acid but he obtained $C_6H_4 \begin{array}{c} CH_2 \\ <\!> \\ SO_2 \end{array} O$ under various conditions. Apparently Jones had hoped that he might obtain additional evidence of the isomers of the chloride of orthosulphobenzoic acid which had been under active investigation in Remsen's laboratory but no definite contribution to this subject appeared.

Jones's first paper from "the Laboratory of Physiological Chemistry of the Johns Hopkins University" was published with Thomas B. Aldrich as senior author and describes the isolation and identification of α-methyl-quinoline as a constituent in the secretion of the anal glands of *M. mephitica*. This was an addition to the list of nitrogenous secretory products.

Many years later W. Hoffman continued work in Jones's laboratory on the secretion of the anal glands of the skunk. Then people in neighboring laboratories recalled the earlier days and were made aware of the inadequacy of the hoods in the chemical laboratory. One bottle of skunk secretion, shipped from West Suffield, Connecticut, was broken in transit and was followed by a letter from the shipper saying:

"I sent one pint and box was proper but I have had H— from Express Co."

Jones next turned to a study of melanins, following an exploration previously made by Abel and Davis. Jones stated in the opening sentence of his first paper: "The name 'Melanin' is a generic term which is used to include all the dark brown or black animal pigments, whether formed in the body by normal processes, or under pathological conditions." The chemistry of these pigments is by no means clear today. Jones's work upon them was largely preparative. By successively destroying other constituents of black horse hair, he prepared material that, when further subjected to a caustic alkali melt, yielded so-called melaninic acid. This was subjected to elementary analysis and

various characterizing tests. A second paper with Auer described the results of its oxidative treatment.

Perhaps it is not inappropriate to speculate upon what might have happened had Jones not found his talents in the next subject to which he turned. Find himself he did in the study of nucleic acids. That he might have continued in one field is not an improbable premise in view of the fact that after his enthusiasm for the study of nucleic acids had been aroused he never published on any subject not directly or indirectly concerned with these substances. That he might have continued his interest in the melanins is not improbable for they furnished an abundant opportunity for exploratory work to which Jones seems to have been adapted. Had he continued the way he had been going, he might have retrod the weary path of preliminary exploration that has proved necessary in the study of nearly every important group of natural substances and that usually comes to a blind alley until the state of the theoretical science attains new power to break through. It is fortunate that he was attracted to the nucleic acids. Enough was known of certain of their constituents to provide a sound basis for systematic chemical work. So much was not known that he was provided an abundance of opportunities. Existing discrepancies and confusions called forth Jones's peculiar talent.

Before considering the specific contributions of Walter Jones to our knowledge of nucleic acids it may be well to outline the state of the subject when he entered the field. Brief as this outline must be, it may aid in recalling the nature of the problems that he attacked and in revealing some of the objectives of his investigations.

The study of nucleic acids begins with the pioneer and classic work of Friederich Miescher. As a student under the anatomist His, Miescher was inspired to look beyond the range of the microscope for the chemical dimensions of morphological peculiarities and he went to Hoppe-Seyler's laboratory with his vision. He saw an opportunity to get at nuclear material by digesting the more easily attacked parts of pus and by this and other methods he obtained powders rich in nuclear material and having chemical properties unlike anything known before. What

Miescher considered to be the characteristic constituent he called "nuclein". In 1869 Miescher submitted a paper on this material to Hoppe-Seyler, the founder and editor of the *Zeitschrift für physiologische Chemie.* Hoppe-Seyler held this paper for confirmation by his students and they soon separated "nuclein" from various sources. In the meantime Miescher was called to Basel and there recognized an opportunity presented by the Rhine salmon. During their ascent of the river the salmon develop their reproductive organs enormously and the males become a potentially huge source of nuclear material since the spermatic fluid, when expressed through the *vas deferens,* consists largely of a suspension of spermatozoa the bulk of which is of nuclear origin. Seizing the opportunity, Miescher prepared from this source samples of "nuclein" of definite and reproducible properties. He was led to believe himself in possession of a salt of a new organic acid and an organic base that he called "protamine". The base was later to be characterized as one of the simplest of the proteins while the organic acid was to become known as nucleic acid.

In 1899, when Jones began his work, a nucleic acid had been separated from the "nuclein" (or nucleoprotein) but "nuclein" rather than its resolved components was still the chief material used in various sorts of investigation.

We must leave much of the detail of Miescher's work to the reviews given by Jones and by Levene and Bass [12] and note that, among the numerous investigators who followed Miescher's pioneer work, the one who was to become Jones's mentor was Albrecht Kossel. Kossel provided Jones a fairly direct intellectual heritage of the field since he had worked under Hoppe-Seyler who in turn had received his start in the field from his painstaking and brilliant student Miescher.

It was Kossel who, in the period 1879 to 1886, first definitely identified purines among the products of the hydrolysis of "nuclein" and recognized their source in the part later to be called nucleic acid rather than in the protamine moiety. It was Kossel who first recognized a pyrimidine in the hydrolysate. With Altmann's preparation in 1889 of the non-protein component,

[12] Jones, *Nucleic Acids* (1920). Levene and Bass, *Nucleic Acids* (1931).

which he called nucleic acid, the material for a more rigid system of constitutional studies was provided. The protamines were to furnish Kossel and others one distinctive group of materials and the nucleic acids another. Jones was to work on the nucleic acids. Although he may have done work on the protamines, he never published it.

With the identification of purines and pyrimidines in the hydrolysates of nuclein and nucleic acid the story of the nucleic acids becomes linked with the story of uric acid which had been more than a century in the making. It is often said that uric acid had the attention of more leading chemists than any other compound. It was discovered in 1776 by Scheele, perhaps the greatest of discoverers in the field of chemistry. Its elementary analysis was made by Liebig, the founder of quantitative organic analysis, and a comprehensive study of its decomposition products was published by Chemistry's Damon and Pythias, Wöhler and Liebig, in 1838. No less a one than von Baeyer undertook and nearly completed one synthesis of uric acid and Emil Fischer completed not only this synthesis but also brought a comprehensive order to the structural relations of the purines as he did for other groups of compounds that are of the greatest bio-chemical importance. While these names claim the attention of every casual student of chemical history, the historian will find many another notable name associated with uric acid, with its close relatives in the purine group and with the next of kin, the pyrimidines. The labors of the great and lesser had provided a very substantial body of information directly applicable to the study of nucleic acids. Nevertheless, this information had to be adapted to the new situation, and while the definitive constitutional studies were still in the making.

In later years it was to be proved that the two better known nucleic acids contain residues of the following substances.

Thymus Nucleic Acid		*Yeast Nucleic Acid*
Phosphoric Acid		Phosphoric Acid
Adenine Guanine	(Purines)	Adenine Guanine
Cytosine Thymine	(Pyrimidines)	Cytosine Uracil
d-2-Desoxyribose	(Carbohydrates)	d-Ribose

Between the beginning of Jones's study (1899) and the identification of d-ribose by Levene and Jacobs ten years passed. Desoxyribose was found after Jones's work was ended. (Levene and London, 1929). Jones himself was never to be concerned extensively with the carbohydrate moieties. Pyrimidines were known as a group and had been dealt with extensively in the study of uric acid; but the first pyrimidine to be isolated from nuclein was thymine, which Kossel and Neumann had found in 1893. The study of its constitution was under way in 1899. Indeed, it was a study contributory to this that Jones was drawn into when he went to Kossel's laboratory as will appear later. Purines, of course, were known as a group and those related to nucleic acids had been found in various natural products; but while the important adenine had been discovered by Kossel in 1885, it was not till 1897 that its relation to uric acid was definitely established by Fischer.

In 1899 it was not proved that the known constituents of a nucleic acid are linked in one molecule. An individual nucleic acid might contain but one purine. There was little vision of how components might be linked together in one nucleic acid. Nucleotides, with the order of linkage phosphate-sugar-purine (or pyrimidine) were unrecognized as pertinent to the studies of nucleic acids. Two nucleotides, inosinic acid (Liebig, 1847) and guanylic acid (Hammarsten, 1894) had been isolated from tissue, but their structures were unknown. Indeed Jones was to mention guanylic acid for several years as a nucleic acid. Nucleosides from sources other than nucleic acids had been known since 1885 but their uses in unravelling the structure of nucleic acids was not fully realized until the work of Levene and Jacobs in 1909 when they were recognized as representing the arrangement sugar-purine (or pyrimidine) in nucleic acid.

Thus a good deal of spade work remained to be done and the absence of those clear-cut constitutional definitions which determine the course of many sorts of investigation left the subject alive with puzzling problems. Perhaps it was because he had been plunged into the midst of these that Jones opened his monograph on the nucleic acids with the remark:

"The early development of nearly every scientific subject is marked by a set of conditions under which it is extremely difficult or even impossible to distinguish the important from the unessential, and unfortunately any misapprehensions which in consequence arise are likely to be so engrafted upon the nomenclature as to perpetuate themselves automatically."

The subject was outlined very dimly during the early days of Jones's work. He found a literature laden with discrepancies. He dealt with material that presented difficulties still felt. There is the unique constitutional complexity. Also it is difficult to prepare nucleic acids in purity sufficient for some of the refined uses of analysis and when this desired end is approached the materials do not lend themselves well to ordinary physicochemical tests for homogeneity, molecular weight, etc. Recollection of all this is essential to an appreciation of Jones's work.

Before a review of Jones's specific contributions, the polemic cloud that hangs over much of his writing must be explained and dispelled. In 1908 Jones said, ". . . the literature on the subject reveals a mass of contradictions, corrections and inconsistencies which it would seem almost impossible to reduce to any satisfactory scientific order." Jones was determined to bring order; he could not endure disorder. Consequently, we find that many of his papers have their immediate objectives in the resolution of discrepancies now forgotten. Were too much emphasis placed upon them, a review could miss the major objectives.

Levene and Bass say of the field in general: "It is singular that in the history of the chemistry of nucleic acids each new conclusion was reached by a path of disagreements, controversies, and errors, and that error often led to progress."

There is a tradition that Jones once ordered the course of a lecture by writing on the black board the names of contributors to the literature of nucleic acids; then crossed out the name of each after discussing his mistakes.

Be that as it may, it was inevitable that the shillalah of this forthright man would occasionally hit a hard head and elicit a reply. To dwell too long upon the resulting controversies, or to pick up each argument where Jones begins on smaller issues, would be to confuse the main issues. The only loss will be to

make less vivid his part as a clarifier, a part too soon forgotten when order in a subject is attained and students of the subject have only to master established relations.

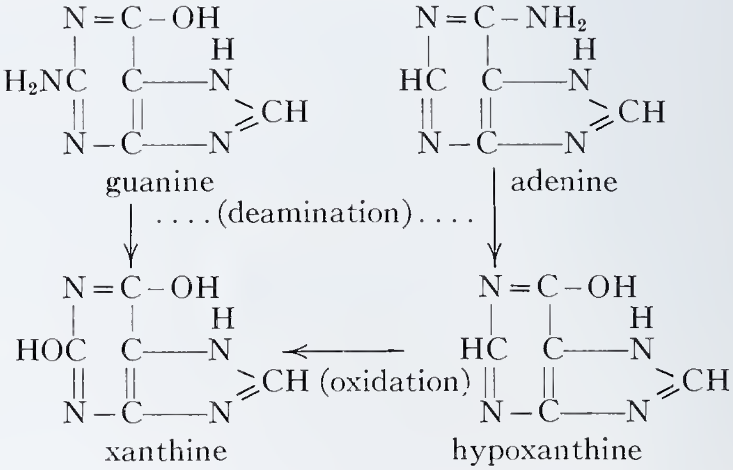
INITIAL RESEARCH ON NUCLEIC ACIDS

As already mentioned, Kossel had isolated thymine from a nuclein hydrolysate but its constitution was not known at the time Jones went to Kossel's laboratory. Jones (1899) prepared a valuable bromine derivative of thymine and confirmed Kossel's opinion that thymine is distinct from the 4-methyl uracil of Behrend.

Then Kossel suggested to Jones the preparation of thymine directly from tissue rather than through the intermediate isolation of the nucleic acid or nuclein. Kossel may have felt what Levene expressed forcefully in a lecture of later years; the advantage of accumulating abundant material before a study of constitution is begun. This was before the development of modern micro methods. Jones (1900) helped to accumulate thymine by successfully preparing it from hydrolysates of herring testicles.

ENZYME STUDIES

In studying the hydrolysates of tissues, of nucleins and even of nucleic acids, various investigators had detected the presence of oxypurines as well as amino purines. For present purposes we may confine attention to the four purines whose relations are exhibited in the following formulas.



Hypoxanthine is formed by deamination of adenine. Xanthine is formed by deamination of guanine or by oxidation of hypoxanthine.

Reports of the occurrence of these four purines and in proportions that maintained no uniform relation had led Kossel to the supposition that there are four nucleic acids each containing the residue of one purine. On the other hand, Schmiedeberg had been led by his study of salmon nucleic acid to the assumption that adenine and guanine occur together in one nucleic acid.

One, who, like the writer, is not intimately acquainted with details of the technical procedures, will have difficulty in judging the extent to which the presence of mononucleotides in tissues confused the issue. Indeed, Jones was later to deal with one aspect of this. But, whatever may have been the extent of this source of confusion, there remained a distinct problem in the relation of these four purines to true nucleic acids. Jones drew the first of the students who were to publish with him on nucleic acids into an examination of this problem. The student was George H. Whipple.

It happened that Abel was then studying the pharmacologically active principle in the medulla of the suprarenal gland. The unused residues furnished Jones and Whipple a source of a nucleic acid that had been reported to contain a methyl purine and to be free of guanine,—unlike any of the nucleic acids from other sources. From their own examination of this material Jones and Whipple (1902) drew the conclusion that the nucleins of the suprarenal glands of sheep and beef are similar to that of the pancreas and that each yields adenine and guanine but no demonstrable amounts of other purines.

Thereafter Jones suspected that the reported xanthine and hypoxanthine might arise from deamination of the amino purines. First, it was advisable to check previous observations. Jones (1904) found that while guanine, adenine and thymine can be identified among the products of hydrolysis of thymus nucleic acid, if acid hydrolysis is used, autolysis of the gland results in the formation of xanthine and hypoxanthine. This was checked by studies of the spleen and suprarenal. Then tissues were

examined for deaminizing enzymes. Jones and Partridge (1904) reported guanase; Jones and Winternitz (1905), adenase.

The justification for inferring specific enzymes for the free purines was chiefly of two sorts; first, the conversion of added guanine to xanthine and the conversion of added adenine to hypoxanthine by infusions of certain tissues; second, failure of the one process and demonstration of the other by specific tissue infusions. Thus, a spleen infusion brought about an alteration of adenine to hypoxanthine, presumably by the action of an adenase, and thence to xanthine in the presence of air, presumably by the action of xanthine oxidase. On the other hand, guanine remained unaltered in this infusion. In contrast to this set of results an extract of pancreas completely changed added guanine to xanthine.

The failure of Jones to emphasize the species from which he obtained the spleen for his experiments led to some amusing and important consequences. Schittenhelm could not confirm Jones's report, but Schittenhelm had used ox spleen while Jones had used pig's spleen. When Jones discovered this he entered a controversy that determined the course of several of his subsequent investigations, for the sharp emphasis given to species and organ specificities led Jones and his students to extensive exploration.

The reader of Jones's papers will note that the initial controversy with Schittenhelm, beginning with the paper by Jones in 1905, is pursued in other directions. Indeed, the full flavor of it developed outside the printed page. Doubtless some of the cryptic remarks that appear in Jones's later papers and in his monograph lack clarity for the reason that their full meaning needed the expansive atmosphere of the lecture room.

The first findings in regard to the distribution of deaminizing enzymes are summarized by Jones in the following diagrams. Let the scheme of enzymatic activities be represented as follows:

$$\begin{tikzcd} & \text{Guanine} \arrow[d, "\text{(Guanase)}"] & \text{Adenine} \arrow[d, "\text{(Adenase)}"] \\ \text{Uric Acid} & \text{Xanthine} \arrow[l, "\text{(Xanthine oxidase)}"'] & \text{Hypoxanthine} \arrow[l, "\text{(Xanthine oxidase)}"'] \end{tikzcd}$$

If a full line arrow represent the indicated enzymatic activity and a dotted arrow represent its absence, distribution may be shown as follows:

Ox liver Pig liver Rabbit liver Dog liver

See Jones and Winternitz (1905), Jones (1905), Jones and Austrian (1906), Jones and Austrian (1907).

That the enzymes are formed successively during embryonic growth was shown by Jones and Austrian (1907), Jones and de Angula (1908) and by others, giving additional presumptive evidence of their individualities. This use of embryos has been followed in other biochemical studies.

Levene and Jacobs (1909-10) then demonstrated the order of linkage in inosinic acid to be: phosphoric acid—pentose—hypoxanthine. By acid hydrolysis they obtained a nucleoside. They discovered its pentose to be the hitherto unknown d-ribose (Levene and Jacobs, 1909-12). Then they extended their methods to yeast nucleic acids and obtained the four nucleosides: guanosine, adenosine, cytidine and uridine. There was now presumptive evidence that yeast nucleic acid is composed of the residues of four nucleotides in which the orders of linkage are:

guanine-ribose-phosphoric acid adenine-ribose-phosphoric acid	} purine nucleotides
cytosine-ribose-phosphoric acid uracil-ribose-phosphoric acid	} pyrimidine nucleotides

There was at hand a constitutional basis upon which to erect a working hypothesis of the enzymatic degradation of yeast nucleic acid.

In the period that follows, Jones recognized that deamination of the purine may occur either while the purine is combined or after it is set free. Having previously reported that guanylic acid is unaffected by pig's pancreas, as judged by the failure of that tissue to catalyze the liberation and deamination of guanine, Jones reinvestigated the matter in 1911 and reported the liberation of phosphoric acid. Therefore, he corrects the interpretation of the previous findings. Guanosine (not guanylic acid)

is unaffected. On the other hand, adenosine is converted to inosine by pig's pancreas, that is, deamination now occurs while the purine is combined.

In the same year Amberg and Jones (1911) showed that although dog's liver forms hypoxanthine from yeast nucleic acid (which would admit of either of two routes) it does not form hypoxanthine from free adenine. Therefore, it was assumed that the liver contains the deaminizing agent, adenosinase, but not the other deaminizing agent, adenase. It also contains the nucleosidase which, if specific, is inosine "hydrolase", but not adenosine "hydrolase".

On the other hand, Amberg and Jones (1913) found that yeast is unable to deaminize adenine whether free or combined. Yeast also shows the specificity of its polynucleotidase by attacking yeast nucleic acid but not thymus nucleic acid.

In Jones's lectures he often emphasized that an enzymatic action upon a specific group may depend upon the mode of linkage of the group carrier with some other residue and he anticipated in some measure what was to be shown subsequently regarding phosporylated metabolites in general.

The recasting of the scheme of enzymatic nucleic acid degradation, initiated by the constitutional investigations of Levene and Jacobs, was also dealt with specifically by Levene and Medigreceanu in 1911, and has since been further elaborated in directions that we need not follow.

The formulation of yeast nucleic acid as a tetranucleotide opened the possibility that a highly selective catalysis of its hydrolysis might result in the formation of dinucleotides or even a trinucleotide as well as mononucleotides. Jones and Richards (1914) believed they had found a dinucleotide resulting from the action of fresh pigs' pancreas on yeast nucleic acid. These "dinucleotides" were described further by Jones and Richards in the following year. The subject was to be followed further in those studies with unorganized hydrolytic agents that will be mentioned in another connection.

In the late period of his work Jones devoted less attention to enzymes. However, in 1920 he described a most remarkable experiment that indicated a thermostable agent in pigs' pancreas by the use of which yeast nucleic acid was split into its four

nucleotides and possibly into dinucleotides initially. (See also Jones, 1922.) Jones and Perkins (1923) returned to this as a means of studying constitution. Levene and Bass (1931, p. 312) state that in unpublished work Levene "was not successful in his attempts to repeat the experiments of Jones". There the remarkable observations on the thermostable agent seems to rest, but the repeated use of the procedure by Jones leaves no doubt that it deserves further study. After the comment on the thermostable agent was written, Jones's observation was confirmed by Dubos and Thompson. See J. Biol. Chem., *124*, 501 (1938).

The series of enzymatic accelerations leading from nucleic acids to uric acid (in man) has an obvious bearing upon the problem of gout and, while this problem involves other phenomena, the clarification of the enzymatic processes in man is a necessary part of the resolution. The few articles in which Jones touches upon this are characteristically confined to items that can be discussed with available experimental data. This is the more remarkable because few other subjects had received more speculative treatments in medical circles. Osler, to be sure, was very cautious but it was not until the 8th edition (1912) of his *Practice of Medicine* that the discussion of the etiology of gout acquired such definiteness as chemistry had provided.

Miller and Jones (1909) examined the Brugsch-Schittenhelm theory with the developing technique of enzyme manipulation. The theory was to the effect that a disturbance in the chain of enzymatic degradation of nucleic acids leads to an accumulation of uric acid. Miller and Jones did find that the organs of a patient (dying of nephritis) who had had gout were unable to destroy uric acid, but Wiechowski had shown that the organs of a gout-free man also lacked uricase. This decisive finding was confirmed by Miller and Jones. Other findings, particularly the absence of adenase in human organs, seems to have suggested to Jones the possibility of tracing some other anomaly of purine metabolism that might have a bearing on gout. The suggestion led to no systematic work on gout.

Leonard and Jones (1909) recognized that the developing knowledge of purine metabolism might account for exogenous uric acid and took cognizance of the developing problem of the

endogenous origins. Burion and Schur had found that perfused muscle produces uric acid at the cost of its hypoxanthine. Observing that the voluntary muscles of the pig, dog and rabbit cannot convert adenine to hypoxanthine, Leonard and Jones assumed that this ruled out nucleic acid as the source of the hypoxanthine known to be present in such muscles and then suggested that this set of data contributes to our knowledge of the source of endogenous uric acid. Voegtlin and Jones (1910) later perfused surviving muscle and found no alteration of adenine.

The production of uric acid in disease is touched upon again by Rohdé and Jones (1910) who found no difference between the enzymatic actions of normal and diseased rat organs. In this same paper endogenous uric acid is again touched upon.

In the article *On the Threefold Physiological Origin of Uric Acid* Jones reviewed the enzymatic studies up to 1910 with particular reference to the subject of gout. He indicates that uric acid may arise from the degradation of ingested nucleic acid, from the hypoxanthine (or its precursor) of the muscle, or through a *de novo* synthesis of the purine ring. Of course, it had been known for a long time that the latter takes place, for instance in starving salmon. Ascoli and Izar in 1908 had given a particular slant to the rôle of uric acid synthesis by claiming that uric acid is destroyed in tissues under aerobic conditions and is resynthesized from the products under anaerobic conditions. Calvery (1927), working in Jones's laboratory, could not confirm the findings of Ascoli and Izar.

Mention already has been made of the fact that gout had long been a subject of wild speculation. In view of this and the fact that Jones was frankly an enthusiast on the subject of purine metabolism one cannot but admire the restraint that he exhibits in his occasional writings on gout, a subject that still remains obscure.

STUDIES BEARING MORE SPECIFICALLY ON CONSTITUTION

As already stated, Jones had contributed to the view that only the residues of adenine and guanine and not those of xanthine and hypoxanthine are native to nucleic acids. An apparent

anomaly was presented by the case of guanylic acid which is now known to be a mononucleotide but which, in 1908, was still called a nucleic acid (see Jones and Rowntree, 1908). The products formed by the hydrolysis of guanylic acid had been in dispute and even the existence of the substance had been denied.

Jones and Rowntree (1908) were able to separate the well-established thymonucleic acid from the guanylic acid of the pancreas as did Levene and Mandal (1908) at the same time. Jones and Rowntree established the wide distribution of guanylic acid. They found guanine to be the only purine liberated on hydrolysis. In the same year Jones compared the nucleic acids of the thymus, spleen and pancreas, utilizing in his experimental work the means he had devised for freeing the preparations from guanylic acid. He gave additional evidence of the identity of this nucleic acid. It is noteworthy that in this paper, where there is resolved the confusion in the literature introduced by contaminating guanylic acid, this substance is spoken of as a "nucleic acid (if it be properly so called)". When Jones returned from further excursion in the enzyme field to the study of constitution, he recognized the general advance in the knowledge of constitution made by Levene and Jacobs and particularly their contribution to our knowledge of guanylic acid and he saw that guanylic acid is a mononucleotide (Jones, 1911) and yeast nucleic acid presumably a tetranucleotide (Jones and Richards, 1914).

Readily accepting the initial and decisive clarification of constitution that Levene and Jacobs had produced, Jones now turned his talents to some isolations projected by the new concepts of structure. From these it was reasonable to suppose that a residue of guanylic acid should be a component of yeast nucleic acid and that suitable hydrolysis of yeast nucleic acid should yield this guanylic acid. Jones in 1912 reported its isolation from a hydrolysate. Jones and Richards, 1914, reported a more detailed study of this substance. It was not emphasized that the constituents of a guanylic acid can be joined in different ways. Consequently, Jones's study of properties was not adequate to prove conclusively that the material isolated from yeast nucleic acid is identical with naturally occurring guanylic acid.

Evidence of the identity was shown later by Levene. Nevertheless, a milestone had been set.

In attempts to hydrolyze a nucleic acid by a partial cleavage that would leave dinucleotides, Jones and Germann (1916) tried Levene and Jacobs's method which consisted of heating the nucleic acid with ammonia at high temperatures. Jones and Germann found reasons for preferring lower temperatures. This and other hydrolytic processes were examined in some detail; stepwise oxidation by permanganate was also tried. By the latter means Jones and Kennedy in 1919 apparently removed the cytosine, uracil and guanine nucleotide groups of yeast nucleic acid and then they isolated for the first time in crystalline form adenine mononucleotide. Another milestone had been set. This isolation of adenine nucleotide had a three-fold significance. From a technical point of view it was an achievement. It joined with the isolation of guanylic acid in confirming the projected polynucleotide structure of yeast nucleic acid. Lastly, this nucleotide was the first of the group of adenine nucleotides to be studied and became the prototype of those compounds that now are seen to be central to several catalytic processes of the cell. Adenine nucleotide was described independently in the same year by Thannhäuser.

Thus there were identified in 1912 and 1919 two purine nucleotides stemming from yeast nucleic acid and giving concrete evidence of the polynucleotide structure of nucleic acid deduced from Levene's work.

In 1914 Jones and Richards reported the hydrolysis of yeast nucleic acid under conditions such that there were obtained "dinucleotides". This, surely, is a theoretical possibility. Its experimental pursuit was to be reported in several papers (Jones and Richards, 1915, Jones and Germann, 1916, Germann, 1916, Jones and Read, 1917, Read and Tottingham, 1917). In reviewing this subject in the second edition of *Nucleic Acids,* Jones remarks "without a single exception, every modern investigator in this field has prepared from yeast nucleic acid a substance which he believed to be a chemical individual containing more than one nucleotide group." While an examination of Jones's own reports leaves a good deal to be desired in the appli-

cation of criteria for individuality, it must be granted that the evidence found in his papers seems adequate to have supported the continuance of research. Where Jones erred was in using the assumed dinucleotides as material for the study of new problems before solving the basic problem of homogeneity. This criticism is so easy to write! Yet with these substances rigid tests have proved very difficult to apply,—so difficult that one wonders what would appear if neglected methods were to be applied to some of the materials reported even recently to be definite compounds. When Levene demonstrated that *certain* preparations of "dinucleotides" could be resolved into fractions each consisting of mononucleotides, Jones accepted this evidence that he had made a mistake in that particular instance, (see Jones and Abt, 1920). Was he *entirely* wrong in general? He implies that he thought dinucleotides to be real in the famous paper on constitution published with Perkins in 1923.

Jones had now entered a period when his genius for clarification had turned from long standing discrepancies in the literature to problems of constitution that were in the making. Mistakes were inevitable but no review of them is adequate without the realization that Jones reported fearlessly what he found and fearlessly admitted mistakes that he was convinced were such.

In 1916 Jones gave a presidential address before the Society of Biological Chemists. In the part that was published he notes that yeast nucleic acid when submitted to 5 per cent sulfuric acid at 100° C. liberates phosphoric acid as if from two sources —the purine nucleotides that release their phosphoric acid residues rapidly and the pyrimidine nucleotides that release their phosphoric acid residues very slowly. In this brief paper there is no very satisfactory analysis of the experimentally observed curve relating phosphoric acid liberated to the time of heating but in a subsequent series of papers individual nucleotides were examined and the results bore out the conclusion that there is a distinct difference in the ease of dephosphorylation of the purine and pyrimidine nucleotides. More exact measurements on rates were to be made later by Levene and Yamagawa. The method was to prove very useful. There is some confusion in following Jones's application of the method because he used

it in dealing with what he supposed to be dinucleotides, and in arguing on modes of linkage. If the central theme be reconstructed with the elimination of these special arguments it reduces to the following. The rate of dephosphorylation of yeast nucleic acid indicates two steps, the first *corresponding to* the rapid dephosphorylation of *isolated* purine nucleotides and the second to the slow dephosphorylation of *isolated* pyrimidine nucleotides. Jones does not deal clearly with possible modifications of rate that might be assumed if some of the phosphoric acid residues, which are terminal in nucleotides, are linked in the nucleic acid itself, although he stated in his 1922 paper on the thermostable agent of pig's pancreas that he had given consideration to this. He leads one to infer that he considered his data proof of the terminal position of the phosphate residues in nucleic acids. This sometimes obscures appreciation of Jones's finding that equal quantities of phosphate are set free in each of the two distinct steps. This latter fact remains one of the outstanding pieces of presumptive evidence that there are equal proportions of pyrimidine and purine nucleotide residues in nucleic acid. In the absence of precise quantitative analytical methods the physico-chemical evidence takes the lead. The method when applied to the elucidation of the modes of linkage led Jones to suggestive but inherently weak arguments.

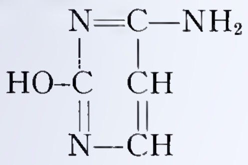
His next argument on structure utilizes some data on the number of free acid groups in yeast nucleic acid—data which Levene promptly criticized.

In 1923 (Jones and Perkins, 1923) Jones protested that he had never defended a formula for the mode of linkage of the nucleotides in yeast nucleic acid, having confined his efforts to "attacks on formulas proposed by others". "Now we change from the offensive to the defensive and propose the following formula." With respect to modes of linkage of nucleotides, with which alone Jones was then concerned, his formula differed from that of Levene (which is now copied in texts) by showing one ether linkage between carbohydrate and carbohydrate. I have established that the evidences in this paper were there *summarized* only after Jones had repeated the experiments again and again. Possible misinterpretations of the observations are

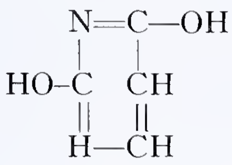
difficult to trace because in neither this nor other related papers are the protocols sufficiently detailed for this purpose. With these remarks I must leave detailed criticism of Jones's evidences to those more familiar with the field. None of the evidences which I happen to have seen for the modes of linkage of nucleotides in nucleic acid appeals to me as rigid proof or more than presumptive and since the evidence before me leaves contradictions that I would not presume to discuss without additional data it would be improper for me to defend or oppose Jones's conclusions.

Perhaps new evidence that certain nucleic acids are large polymers of nucleotides will force reconsideration of data that hitherto have been interpreted under the presumption of a tetranucleotide constitution.

In one of the last of Jones's publications he turns to the question of whether there is a parallelism between the deamination of purines and the deamination of pyrimidines during treatments of nucleic acids. The cytosine and uracil reported in yeast nucleic acid stand in the following relationship.



Cytosine



Uracil

Jones and Perkins (1925), on hydrolysis of yeast nucleic acid with 1 per cent NaOH solution, failed to find the uracil nucleotide. They say: "In so far as this result is of value the conclusion is obvious that the oxypyrimidine derivatives (uracil, uracil nucleoside, and uracil nucleotide) are not referable to an oxypyrimidine group in nucleic acid but are secondary products formed during hydrolysis by deaminization of the corresponding cytosine derivatives or their precursors."

Levene and Bass (1931, p. 276) refer to this paper as reviving the trinucleotide theory of yeast nucleic acid. If so, it was by inference, for no statement to that effect occurs in the paper.

Later Jones and Calvery (1927) discovered that the "failure

of Jones and Perkins and of Calvery to isolate uracil nucleotide from the hydrolytic products of yeast nucleic acid was due to a loss of material". By use of ammonia hydrolysis they obtained an hydrolysate from which all of the expected nucleotides were isolated.

It should not be overlooked that the failure to find uracil had been a *fact* that stood not only against accepted views but also against a group of evidences to which Jones himself had contributed. The publication might be said to have been premature but it cannot be denied that it was courageous. The withdrawal replaced Jones in the rôle in which he was supreme—the resolver of discrepancies—this time one originating in his own work.

NOTES ON MISCELLANEOUS PARTS OF JONES'S WORK

There were four papers published with Gamgee on the optical activity of nucleic acid and nucleoproteins. These papers were published in 1903. They bear internal evidence of Gamgee's authorship but that Gamgee was the author this writer has no proof. Aside from an abundance of entertaining remarks the papers present the then interesting observations that the nucleoproteins are unlike most proteins other than hemoglobin in that they are dextrorotatory, that the specific rotation of nucleic acid differs from that of the nucleoprotein, and that the specific rotations vary with the acidity of the solutions. Later Jones (1908) was to use the identity of specific rotations under fixed conditions and the uniformity of their variations with changes of the acidity of the solutions as an argument that the nucleic acids of the thymus, spleen and pancreas are identical. The optical properties of solutions of nucleic acids were to be referred to occasionally as, for example, by Amberg and Jones (1911), but were not extensively used. The change of rotation with change of the solution, especially its acidity, was a very important observation.

Since Jones was associated with Kossel at a time when the separation and analytical determinations of purines and pyrimidines were developing and since most early methods of sepa-

rating nucleic acids were empirical and their improvements dependent upon accretions of experience, it is difficult to appraise the originality of some of Jones's contributions to preparation and analyses. These aspects are so seldom emphasized in Jones's papers as to lead one to believe that, while he contributed his part, he would not have claimed a large portion of credit. In a letter to C. A. Morrow he writes:

"I do not know whether or not the description given in my monograph for the preparation of guanine and adenine from yeast nucleic acid is original. I have never seen it described in just this form anywhere. But you have to take into consideration that the preparation of guanine and adenine is very much more difficult with animal nucleic acid than with plant nucleic acid and the earlier descriptions for the preparation of the two bases applied to the more difficult preparation from thymus nucleic acid."

The two papers on phosphorus determination (1916, 1923) may be regarded as indulgencies. It is often said of a good analyst that he can do better with a poor method than a poor analyst can do with a good method. If Jones preferred to dust a dried precipitate of ammonium magnesium phosphate from the filter paper and discard the paper rather than to ignite and convert the phosphate to pyrophosphate it was doubtless his privilege and it was evidently his joy to show a reliability consonant with his own requirements.

In the closing years of Jones's career he was entering a field for which he was eminently suited—the examination of tissues for new material related to the compounds of his earlier studies. Mention has already been made of his studies of the naturally occurring guanylic acid, and of his interest in inosinic acid. Had space permitted we would have discussed his occasional concern with "β-nucleoproteins," substances or mixtures some of which had yielded guanylic acid. In 1922 Jones and Perkins turned their attention to those nucleotides in animal tissues that differ from the nucleotides of "animal" nucleic acid in having the pentose of "plant" nucleic acid. They say: "We were formerly inclined to believe that the presence of plant nucleotides in animal tissues is caused by the plant food which the animal consumes. But the tentative and confessedly inadequate evidence upon

which this view was based has since been found erroneous." Jones and Perkins then recovered from the "β-nucleoprotein" of the pancreas not only guanine nucleotide but also cytosine and adenine nucleotides that bore every resemblance to the corresponding *ribo*-nucleotides from yeast nucleic acids. The inference was that a *ribo*-nucleic acid is native to the animal, a view later confirmed by Jorpes. Said Jones: "It thus seems more than probable that the distinction between animal and plant nucleic acid will in the future not be so definitely drawn." Up to that time the distinction had been drawn sharply.

The reviving spirit of exploration that radiated from Walter Jones was made evident by unpublished examinations of corn and wheat "germ flours" and tubercle bacilli, by the note of Shaffer, Folkoff and S. Bayne-Jones on the nucleic acids of bacteria, by Calvery's examination of tea leaves, etc. Important contributions from Jones's laboratory were Buell's isolation of an oxyadenine from blood and her evidence that inosinic acid of muscle can originate in the adenine nucleotide.

Throughout his career Walter Jones counseled his assistants to pursue their own problems; but of the later period it may be said that he could not hold them from enthusiastic participation in cooperative and independent researches within his field. Of course, it is impossible to say what might have come out of his laboratory had not his illness cut short his own work and finally altered his staff. Nevertheless, it is clear that hands were getting close to remarkable substances that it was given to others to discover. Let imagination play with the dream of Walter Jones's enthusiasm could he now see that the materials which his hands almost touched are linking hitherto unrelated realms of research —catalysis of phosporylation, catalysis of oxidation-reduction, vitamins of the B group, also the structure of chromosomes. Again the pursuit of understanding for its own sake, from which Walter Jones would not deviate, bids fair to place in the hands of the physician more power than frontal attacks have yielded.

Jones's Monograph, *Nucleic Acids,* the first edition of which was published in 1914 and the second in 1920, remained for many years the only comprehensive review available in English. It dealt briefly with the historical background and set forth the

chemistry of the then known components of nucleic acids. Structures were discussed. Properly, a good deal of attention was given to the enzymatic transformations which Jones had studied extensively. The appendix gave invaluable directions for important preparations and there was a good bibliography.

Arthur Harden, reviewing the second edition in 1921, wrote "To the biochemist this book cannot fail to be of profound interest, alike for the importance of the matter and the lucidity of the exposition." With me Walter Jones left the impression that while he remained modest in his claims for the monograph he considered its consolidation of knowledge his duty as a scholar.

From time to time Jones contributed to various books and reviews.

Professor Abel had written several of the reviews of physiological chemistry for Gould's American Year-Book of Medicine and Surgery and from 1900 to 1905 Walter Jones and Reid Hunt together took over this laborious duty. These reviews were occasionally punctuated with spicy remarks. Examples:

"Morner's discovery of two isomeric forms of cystin as hydrolytic products of horn may furnish food for reflection to the artists who are accustomed to draw pictures of the proteid molecule without giving the sulphur atom any consideration."

"By a curious mixture of good chemic argument and unwarranted assumption, the authors (Nencki and Zaleski) arrive at an appalling structure formula for hemin."

"The discovery of an analytic method always marks an epoch in chemical development. . . . Fischer now introduces a method of separating the hydrolytic products of proteids . . ." (Fischer's famous ester method described at length).

The yearly comments on the developing knowledge of epinephrine, in which Abel kept the lead, are especially interesting and historically important.

The following bibliography of Walter Jones's scientific work is, I believe, complete except possibly the list of minor notices. Its order conforms in sequence to the numbering discovered in Jones's own, incomplete set of reprints and it contains titles not found in Jones's list of his papers,—the only document pertaining to his scientific work that was found among his effects after his death.

BIBLIOGRAPHY OF WALTER (JENNINGS) JONES

Separates, Books, and Contributions to Books

Walter Jones. Sulphon-phthaleins Derived from Ortho-sulpho-para-toluic Acid. Dissertation, Johns Hopkins University. Baltimore. 1891. Press of Isaac Friedenwald Co.

Anonymous. Supplementary Notes to Laboratory Work in Chemical Toxicology for the Use of Students in the Johns Hopkins Medical School. Privately printed. (No date.) [Note: Professor J. J. Abel, in whose laboratory it was used, states that this was written by Walter Jones.]

Walter Jones and D. Wright Wilson. A Laboratory Manual of Physiological Chemistry. Johns Hopkins University, Baltimore. (No date.) Printed at the Waverly Press, Baltimore.

Walter Jones. Nucleic Acids, Their Chemical Properties and Physiological Conduct. First edition, 1914; second, 1920. Monographs on Biochemistry. Edited by R. H. A. Plimmer and F. G. Hopkins. Longmans, Green and Co.

Reid Hunt and Walter Jones. Non-alkaloidal Organic Poisons. In A Text-Book of Legal Medicine and Toxicology. Edited by Frederick Peterson and Walter S. Haines. Vol. II, p. 532. W. B. Saunders and Co., Philadelphia, New York and London, 1904.

Walter Jones and Reid Hunt. Physiological Chemistry. (Reviews.) In The American Year-book of Medicine and Surgery. Edited by George M. Gould. Published by W. B. Saunders.

1900 Volume *Medicine,* p. 562.
1901 Volume *Medicine,* p. 612.
1902 Volume *Medicine,* p. 648.
1903 Volume *Medicine,* p. 638.
1904 Volume *Medicine,* p. 601.
1905 Volume *Medicine,* p. 620.

Walter Jones. Nucleic Acids. In Endocrinology and Metabolism. Edited by Lewellys F. Barker. Vol. 3, p. 134. Appleton and Co., 1922.

Journal Articles

W. E. Stone and Walter J. Jones. The digestibility of the pentose carbohydrates. Agri. Sci., 7, 6, 1893.

Walter Jones. A reduction product of orthosulphobenzoic chloride. Am. Chem. J., 16, 366, 1894.

Walter Jones. Investigations on the sulphonphthaleins. VI. Sulphonphthaleins derived from orthosulphoparatoluic acid. Am. Chem. J., 17, 556, 1895.

T. B. Aldrich and Walter Jones. α-Methyl-quinoline as a constituent of the secretion of the anal glands of mèphitis mephitica. J. Exptl. Med., 2, 439, 1897.

Walter Jones. The chemistry of the melanins. Am. J. Physiol., 2, 380, 1899.

Walter Jones. Ueber das Thymin. Ztschr. physiol. Chem., 29, 20, 1899.

Walter Jones. Ueber die Darstellung des Thymins. Ztschr. physiol. Chem., 29, 461, 1900.

Walter Jones and J. Auer. On the oxidation of native pigments. Am. J. Physiol., 5, 321, 1901.

Walter Jones and G. H. Whipple. The nucleoproteid of the suprarenal gland. Am. J. Physiol., 7, 423, 1902.

A. Gamgee and Walter Jones. On the nucleoproteids of the pancreas, thymus and suprarenal glands, with especial reference to their optical activity. Proc. Roy. Soc., 71, 385, 1903.

A. Gamgee and Walter Jones. On the optical activity of the nucleic acid of the thymus gland. Chem. News, 87, 303; Proc. Roy. Soc., 72, 100, 1903.

A. Gamgee and Walter Jones. Über die Nucleoproteide des Pankreas, der Thymus und der Nebenniere, mit besonderer Berücksichtigung ihrer optischen Aktivitat. Beiträge zur chem. Physiol. [Hofmeister] 4, 10, 1903.

A. Gamgee and Walter Jones. On the nucleoproteids of the pancreas, thymus, and suprarenal gland, with especial reference to their optical activity. Am. J. Physiol., 8, 447, 1903.

Walter Jones. Über das Enzym der Thymusdrüse. Ztschr. physiol. Chem., 41, 101, 1904.

Walter Jones. Über die Selbstverdauung von Nucleoproteiden. Ztschr. physiol. Chem., 42, 35, 1904.

Walter Jones and C. L. Partridge. Über die Guanase. Ztschr. physiol. Chem., 42, 343, 1904.

Walter Jones and M. C. Winternitz. Über die Adenase. Ztschr. physiol. Chem., 44, 1, 1905.

Walter Jones. Über das Vorkommen der Guanase in der Rindermilz und ihr Fehlen in der Milz des Schweines. Ztschr. physiol. Chem., 45, 84, 1905.

Walter Jones and C. R. Austrian. Über die Verteilung der Fermente des Nucleinstoffwechsels. Ztschr. physiol. Chem., 48, 110, 1906.

Walter Jones and C. R. Austrian. On thymus nucleic acid. J. Biol. Chem., 3, 1, 1907.

Walter Jones and C. R. Austrian. On the nuclein ferments of embryos. J. Biol. Chem., 3, 227, 1907.

Walter Jones and L. G. Rowntree. On the guanylic acid of the spleen. J. Biol. Chem., 4, 289, 1908.

Walter Jones. On the identity of the nucleic acids of the thymus, spleen, and pancreas. J. Biol. Chem., 5, 1, 1908.

M. C. Winternitz and Walter Jones. Über den Nucleinstoffwechsel mit besonderer Berücksichtigung der Nucleinferments in den menschlichen Organen. Ztschr. physiol. Chem., 60, 180, 1909.
M. C. Straughn and Walter Jones. The nuclein ferments of yeast. J. Biol. Chem., 6, 245, 1909.
V. N. Leonard and Walter Jones. On preformed hypoxanthin. J. Biol. Chem., 6, 453, 1909.
J. R. Miller and Walter Jones. Über die Fermente des Nucleinstoffwechsels bei der Gicht. Ztschr. physiol. Chem., 61, 395, 1909.
A. Rohdé and Walter Jones. The purin ferments of the rat. J. Biol. Chem., 7, 237, 1910.
Walter Jones. Über die Beziehung der aus wässerigen Organextrakten gewonnenen Nucleinfermente zu den physiologischen Vorgängen im lebenden Organismus. Ztschr. physiol. Chem., 65, 383, 1910.
C. Vögtlin and Walter Jones. Über Adenase und ihre Beziehung zu der Entstehung von Hypoxanthin im Organismus. Ztschr. physiol. Chem., 66, 250, 1910.
Walter Jones. On the threefold physiological origin of uric acid. Johns Hopkins Hospital Bull., 21, 240, 1910.
G. DeF. Barnett and Walter Jones. On the recovery of adenine. J. Biol. Chem., 9, 93, 1911.
Walter Jones. Concerning nucleases. J. Biol. Chem., 9, 129, 1911.
Walter Jones. On the physiological agents which are concerned in the nuclein fermentation, with special reference to four independent desamidases. J. Biol. Chem., 9, 169, 1911.
S. Amberg and Walter Jones. On the application of the optical method to a study of the enzymatic decomposition of nucleic acids. J. Biol. Chem., 10, 81, 1911.
S. Amberg and Walter Jones. Über die bei der Spaltung der Nucleine in Betracht kommenden Fermente mit besonderer Berücksichtigung der Bildung von Hypoxanthin in der Abwesenheit von Adenase. Ztschr. physiol. Chem., 73, 407, 1911.
Walter Jones. On the formation of guanylic acid from yeast nucleic acid (preliminary communication). J. Biol. Chem., 12, 31, 1912.
S. Amberg and Walter Jones. The action of yeast on yeast nucleic acid. J. Biol. Chem., 13, 441, 1913.
Walter Jones and A. E. Richards. The partial enzymatic hydrolysis of yeast nucleic acid. J. Biol. Chem., 17, 71, 1914.
Walter Jones and A. E. Richards. Simpler nucleotides from yeast nucleic acid. J. Biol. Chem., 20, 25, 1915.
Walter Jones. An indirect method of determining pyrimidine groups in nucleotides. Presidential address, American Society of Biological Chemists. J. Biol. Chem., 24, Proc. iii, 1916.
Walter Jones. The admissibility of ammonium magnesium phosphate as a form in which to weigh phosphoric acid. J. Biol. Chem., 25, 87, 1916.

Walter Jones and H. C. Germann. Hydrolysis of yeast nucleic acid with ammonia. J. Biol. Chem., 25, 93, 1916.
Walter Jones and B. E. Read. Adenine-uracil dinucleotide and the structure of yeast nucleic acid. J. Biol. Chem., 29, 111, 1917.
Walter Jones and B. E. Read. The mode of nucleotide linkage in yeast nucleic acid. J. Biol. Chem., 29, 123, 1917.
Walter Jones and B. E. Read. Uracil-cytosine dinucleotide. J. Biol. Chem., 31, 39, 1917.
Walter Jones and B. E. Read. The structure of the purine mononucleotides. J. Biol. Chem., 31, 337, 1917.
Walter Jones and R. P. Kennedy. Adenine mononucleotide. J. Pharm. Exptl. Therap., 12, 253; 13, 45, 1918 and 1919.
Walter Jones. The application of the Kjeldahl method to compounds of brucine, with reference to the brucine salt of a new nucleotide. J. Pharm. Exptl. Therap., 13, 489, 1919.
Walter Jones and A. F. Abt. The preparation of adenine nucleotide by hydrolysis of yeast nucleic acid with ammonia. Am. J. Physiol., 50, 574, 1920.
Walter Jones. The chemical constitution of adenine nucleotide and of yeast nucleic acid. Am. J. Physiol., 52, 193, 1920.
Walter Jones. The action of boiled pancreas extract on yeast nucleic acid. Am. J. Physiol., 52, 203, 1920.
Walter Jones. The thermostable active agent of pig's pancreas. J. Biol. Chem., 50, 323, 1922.
Walter Jones and C. Folkoff. The isolation of nucleic acid from tissues. Johns Hopkins Hospital Bull., 33, 443, 1922.
Walter Jones and M. E. Perkins. The preparation of nucleotides from yeast nucleic acid. Johns Hopkins Hospital Bull., 34, 63, 1923.
Walter Jones and M. E. Perkins. The gravimetric determination of organic phosphorus. J. Biol. Chem., 55, 343, 1923.
Walter Jones and M. E. Perkins. The nucleotides formed by the action of boiled pancreas on yeast nucleic acid. J. Biol. Chem., 55, 557, 1923.
Walter Jones and M. E. Perkins. The formation of nucleotides from yeast nucleic acid by the action of sodium hydroxide at room temperature. J. Biol. Chem., 55, 567, 1923.
Walter Jones and M. E. Perkins. The occurrence of plant nucleotides in animal tissues. J. Biol. Chem., 62, 291, 1924.
Walter Jones and M. E. Perkins. The nitrogenous groups of plant nucleic acid. J. Biol. Chem., 62, 557, 1925.
H. O. Calvery and Walter Jones. The nitrogenous groups of nucleic acid. J. Biol. Chem., 73, 73, 1927.

Minor Notices

Walter Jones. On the enzyme of the thymus. Am. J. Physiol., 10, xxiv, 1903.

Walter Jones. On the enzyme of the suprarenal gland. Am. J. Physiol., 10, xxv, 1903.
Walter Jones and C. R. Austrian. The occurrence of ferments in embryos. J. Biol. Chem., 3, Proc. xxviii, 1907.
Walter Jones and J. de Angulo. Studies in comparative physiological chemistry. J. Biol. Chem., 6, Proc. xlv, 1909.
Walter Jones and G. DeF. Barnett. On the recovery of adenine. J. Biol. Chem., 9, Proc. xix, 1911.
Walter Jones. On a specific nuclease from guanylic acid. J. Biol. Chem., 9, Proc. xxviii, 1911.
Walter Jones and Riley. [Mentioned by Jones in Nucleic Acids, 2nd ed., p. 43; but see Presidential Address, Jones, 1916.]

Papers, by Jones's assistants, that pertain to his major subject of research, and that were published from his laboratory.

E. K. Marshall, Jr. On the self digestion of the thymus. J. Biol. Chem., 16, 81, 1913.
H. C. Germann. The partition of phosphorus in thymus nucleic acid. J. Biol. Chem., 25, 189, 1916.
B. E. Read. Guanine mononucleotide (Guanylic acid) and its preparation from yeast nucleic acid. J. Biol. Chem., 31, 47, 1917.
B. E. Read and W. E. Tottingham. Tritico nucleic acid. J. Biol. Chem., 31, 295, 1917.
D. W. Wilson. Studies in pyrimidine metabolism. Proc. Soc. Exptl. Biol. Med., 17, 179, 1920; J. Biol. Chem., 56, 215, 1923.
W. S. Hoffman. The isolation of adenine nucleotide from blood. Johns Hopkins Hospital Bull., 35, 417, 1924.
W. S. Hoffman. The isolation of crystalline adenine nucleotide from blood. J. Biol. Chem., 63, 675, 1925.
H. O. Calvery. The preparation of adenine nucleotide from tea leaves. J. Biol. Chem., 68, 593, 1926.
M. V. Buell and M. E. Perkins. Crystalline guanine nucleotide. J. Biol. Chem., 72, 21, 1927.
H. O. Calvery. The action of sodium carbonate on yeast nucleic acid. J. Biol. Chem., 72, 27, 1927.
H. O. Calvery. The chemistry of tea leaves. II. The isolation of guanine nucleotide and cytosine nucleotide. J. Biol. Chem., 72, 549, 1927.
M. V. Buell and M. E. Perkins. Oxyadenine. J. Biol. Chem., 72, 745, 1927.
W. S. Hoffman. The micro determination of pentose in yeast nucleic acid and its derivatives. J. Biol. Chem., 73, 15, 1927.
H. O. Calvery. A note on the enzyme uricase. J. Biol. Chem., 73, 77, 1927.
H. O. Calvery and D. B. Remsen. The nucleotides of triticonucleic acid. J. Biol. Chem., 73, 593, 1927.

M. V. Buell and M. E. Perkins. Adenine nucleotide content of blood with a micro analytical method for its determination. J. Biol. Chem., 76, 95, 1928.

H. O. Calvery. The isolation of four pentose nucleotides from chicken embryos. J. Biol. Chem., 77, 489, 1928.

M. V. Buell. On the origin of inosinic acid. J. Biol. Chem., 85, 435, 1930.

Henry Mitchell

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OF

HENRY MITCHELL

1830–1902

BY

H. A. MARMER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

HENRY MITCHELL

1830–1902

BY H. A. MARMER

Henry Mitchell was born on Nantucket Island, Mass., on September 16, 1830, the fourth son in a family of four sons and six daughters of William and Lydia (Coleman) Mitchell. The family was of Quaker stock long settled in America. His mother had been a school teacher prior to her marriage. His father, too, had been a teacher, but later became a bank official and one of the Overseers of Harvard College. Interested especially in astronomy, the father enjoyed a wide acquaintance among American men of science.

At this time Nantucket ranked after Boston and Salem as the third commercial city of the State, its prosperity being due primarily to the whaling industry. Nantucket whalers were ranging the seas from the Arctic to the Antarctic, and this fostered in the island community the study of navigation, including mathematics and astronomy. Henry Mitchell's mother used to relate that in her infancy the little children were taught to box the compass in the "Monthly Meeting School" in place of the catechism.

Young Mitchell obtained his education in private schools and at home where his immediate family furnished good examples and excellent instructors. The well-known astronomer Maria Mitchell was his sister, twelve years his senior. During his youth she was librarian of the Nantucket Athenaeum and was busily engaged in her astronomical labors, sharing her father's enthusiasm for astronomy. Under such conditions Henry Mitchell received a sound training.

In 1849, being then nineteen years of age, he entered the United States Coast Survey. His first assignment was in connection with triangulation, but later he was transferred to the hydrographic branch. Here his abilities soon became manifest. In 1854 he was entrusted with carrying out a tidal survey of Nantucket and Vineyard Sounds. This involved simultaneous observations at different places along the outer and inner

coasts to determine the progress of the tide from the open sea. Difficulties were experienced in the construction of tide gages to withstand the force of the currents and of the breakers at the outside stations. Mitchell was successful in devising a special form of tide gage which could be used on the open sea coast and in situations exposed to strong currents. This he described and illustrated in the appendix to the "Report of the Superintendent of the U. S. Coast Survey" for 1854.

For the following two years he was engaged in studying the complicated system of tides and currents in Nantucket and Vineyard Sounds and in elucidating their movements. In appendices to the Coast Survey Reports for 1856 and 1857, he sketches briefly the salient features of the tides and currents in these waterways. In the latter report, too, he describes a spar form of tide gage for observing tides in deep water and in situations exposed to heavy seas, that he used during the year.

In 1856 he began an investigation of the tides and currents in New York Harbor and vicinity. In this investigation Mitchell had two aims in view: first, the securing of data which would permit the prediction of the tides and currents in the localities in question for the use of the mariner; second, the study of tides and currents as agents of geologic change in molding and changing shore lines and harbors.

In New York Harbor, the tidal phenomena are complicated by the fact that the tide enters the harbor by two channels, one past Sandy Hook, and the other through Long Island Sound. Due to this, very strong currents are brought about in the East River, especially in the vicinity of Hell Gate, with very rapid changes in the time of tide through that waterway.

For studying the subsurface currents, Mitchell devised a simple and ingenious apparatus. To quote his own words "This consists of two large copper globes, as floats, connected by a slender cord, one weighted so as to float when immersed to the depth of four feet, and the other so as to sink to different depths in the currents which it may be desired to investigate. The motion of the apparatus will depend, of course, upon the difference of movement at four feet, the nearly superficial current,

and below, so that, obtaining by the ordinary log the movement at the surface, that below becomes known by observing the motion of the apparatus itself."

The investigation and elucidation of the tidal phenomena in New York Harbor was Mitchell's principal work up to and including the year 1859. Interim reports on the results of these investigations were published in the annual reports of the Coast Survey each year and in the 1859 Report he published an appendix on "The Physical Survey of New York Harbor and the Coast of Long Island, with Descriptions of Apparatus for Observing Currents." In this appendix he summarized briefly the relations existing between the currents and shoals in this region, and described several different forms of apparatus for measuring currents and also a new shape devised for a pile to be used as a support for a tide gage.

Beginning with 1860, Mitchell's attention was turned to the problems involved in the maintenance of the channels of Boston Harbor. In the Coast Survey Report for 1860 he published a discussion of the tides and currents in Boston Harbor. In the same Report he also described two instruments he devised for collecting bottom specimens in alluvial harbors.

Hydrographic surveys and studies of tides and currents along the New England coast and in New York Harbor engaged his attention up to 1866. In 1862 he also made a survey of Oregon and Hatteras Inlets, North Carolina, at the request of the military authorities in connection with the work of the North Atlantic blockading squadron. And in 1864, at the request of the Navy Department, he studied the action of floating ice in Delaware Bay.

In 1866 he enlarged his experience of hydrography to include the southeastern coast of the United States. In connection with a proposed cable uniting the United States and Cuba, he made soundings across the Straits of Florida, embodying the results of his observations in an appendix to the Coast Survey Report for the above-mentioned year. In this Report he also dealt with the difficulties in the way of laying the proposed cable. This work was continued during the following two years, the project

being enlarged to secure data beyond the immediate needs for cable laying so as to derive a better knowledge of the Gulf Stream.

At this time it was thought that a polar countercurrent existed below the northerly setting Gulf Stream, the low temperature of the deeper waters appearing to confirm this theory. Mitchell himself had accepted this polar current theory in a paper presented before the National Academy of Sciences in 1866. But in the Coast Survey Report for 1867 he challenges this theory, stressing the fact that between Cuba and Florida *"the Gulf Stream has a nearly uniform velocity, and constant course for a depth of six hundred fathoms, although its temperature varies in this depth 40° Fahrenheit."*

The growth of commerce, coupled with the increased draft and size of ships, was at this time bringing the problems connected with harbor improvement to the fore. Desiring to learn the latest developments in European practice, it was decided to send an American engineer to Europe. Although still a comparatively young man at this time, Mitchell was already recognized as an authority in hydrographic matters, and it was therefore natural that he should be chosen. Leaving in May of 1868, he visited Germany, Holland, England, France and Italy, studying canals and harbors and conferring with leading engineers. He also visited Egypt for the purpose of inspecting the Suez Canal which was nearing completion.

In February of 1869 he returned to the United States, and in the *North American Review* for October of that year he published a paper on "The Coast of Egypt and the Suez Canal." In this paper of some thirty odd pages there is not only an engineer's description of a great engineering undertaking, but also the reflections of a scientist on the hydrographic features of a region that posed a variety of problems relative to its development. The concluding paragraph of this paper furnishes a good example both of Mitchell's vigorous style and also of his interest in public matters outside the narrow specialized field of his own chosen profession.

"I have looked in vain through the entire history of this French enterprise in Egypt, to discover the least trace of

earnest effort or sincere co-operation on the part of the Egyptian or Turkish government. I believe that the Viceroys of Egypt, from Mahomet Ali down to the present weak prince, have been coaxed into acquiescence by the master minds that conceived and executed this brilliant work, and I am convinced that this costly avenue, and the commerce employing it, will never be secure from interruption till the territory is neutralized or otherwise wrested from Mohammedan misrule."

Another study resulting from his European investigations, and one of wider scope than the preceding, appeared as an appendix to the Coast Survey Report for 1869 under the title "On the Reclamation of Tide-Lands and Its Relation to Navigation." In this paper he formulates the relations existing between tidal and nontidal currents in regard to channel scour, and the effects of various types of reclamation works.

From his return in 1869 up to 1874 Mitchell was engaged principally with surveys and studies connected with the problems of New York Harbor and harbors along the New England coast. In the summer of 1870 he was also called to the Pacific coast to study the probable effects of extending piers in certain channels of San Francisco Bay. Interim reports on the progress of these studies appeared in the annual reports of the Coast Survey. In the Report for 1871 he also published a paper on "Hints and Suggestions Upon the Location of Harbor-Lines."

Early in 1874 he was appointed a member of the Commission on the construction of an Oceanic Ship Canal and made a personal inspection of suggested routes for the proposed canals through Nicaragua and the Isthmus of Darien. In that same year President Grant appointed him a member of the Board of Engineers to survey the mouth of the Mississippi River. The following year, at the request of James B. Eads, he served as a member of an Advisory Board in connection with the construction of the Mississippi Jetties. From 1875 to 1877 he also served on the Advisory Board to the Harbor Commissions of Virginia and Rhode Island, and in 1879 he was appointed a member of the Mississippi River Commission by President Hayes.

In connection with his work on tides, Mitchell studied the

question of the alleged emergence of the northeastern shores of the American Continent, this thesis being supported by eminent geologists. In a paper entitled "Notes Concerning Alleged Changes in the Relative Elevations of Land and Sea" in the Coast Survey Report for 1877, he made a critical study of the records upon which this thesis was based, and showed conclusively that it was untenable.

Delaware River, forming one of the important waterways of the country, presented numerous problems in connection with harbor improvement. In 1877 a hydrographic survey of this river was made under Mitchell's direction, and in the Report for 1879 he published an appendix under the title "On a Physical Survey of the Delaware River in Front of Philadelphia." Here he gave not only the results of the survey in detail, but also a general dissertation on channels in tidal rivers and the principles involved in the maintenance of such channels.

As a member of the Mississippi River Commission, he studied various problems connected with the Mississippi River. In the Coast Survey Report for 1882, he published a study on the effects of river bends in the lower Mississippi, in which he concluded that bends, on the whole, offer no advantages.

Surveys and studies of the harbors of New York and Philadelphia continued for the next few years, problems in connection with shoals becoming especially important. A careful study of Monomoy and its shoals by Mitchell appeared in the Coast Survey Report for 1886. And in that Report he also published an appendix "On the Circulation of the Sea Through New York Harbor" in which he called attention to the fact that "although the tidal currents of New York, especially in the East River, appear to move to and fro, with ebb and flood, in monotonous repetition, like the swing of a pendulum, there is a *net gain,* under ordinary conditions of river discharge, to the westward, *i.e.*, a permanent transfer of water from the Sound through the harbor and out into the ocean over Sandy Hook Bar." This was a very important discovery, both in elucidating the complex tidal movements in the harbor and in its practical applications in connection with the improvement of the harbor.

In May of 1888 he presented his resignation from the Coast Survey to take effect at the close of work on June 30, having served continuously for 39 years. During this time his attainments had received recognition in the world outside his chosen profession. Various scientific societies honored him by election to membership. In 1867 Harvard College conferred upon him the degree of Master of Arts; in 1869 he was offered the Professorship of Physical Hydrography at the Massachusetts Institute of Technology, and in 1873 the same chair was offered him in the Agassiz School of Science. In 1875 he was elected to the National Academy of Sciences.

Mitchell was married three times. His first wife was Mary Dawes, of Boston, to whom he was married in his early twenties and who left him a widower after twelve years. In 1873, he was married to Margaret Hayward who died in 1875, about five months after the birth of a daughter, his only child. Two years later, he married his deceased wife's elder sister, Mary Hayward. On resigning from the Coast Survey, he led a retired, studious life, spending his summers in Nantucket and his winters near Boston. In 1890, he served as a member of the Commission of the Annual Assay of the Mint; in 1893, he was appointed a member of the Advisory Council of the World's Columbian Water Commerce Congress; and in 1896, he published in the *Proceedings* of the American Academy of Arts and Sciences a biographical sketch of Ferdinand de Lesseps.

A year after his resignation he was offered the Superintendency of the Coast Survey by President Harrison. But Mitchell's health did not permit him to assume the burdens of the office, and he declined the offer. In March 1902 his wife died, following which he made his home with his daughter in New York City, where he died, December 2, 1902.

By a person who knew him in his later life he was described as "a charming and interesting man who shared the characteristics of his sister Maria in an ability to interest his friends wherever he went, conversing with great ease, and much ability to see the humorous side of life. He continued to dress in the style of his time, changing but little with fashion."

BIBLIOGRAPHY

NOTE: The greater number of Mitchell's publications appeared in the annual reports of the Superintendent of the United States Coast Survey. In 1878 the name of the bureau was changed to United States Coast and Geodetic Survey. In this bibliography the reports are specified by the year for which issued and not the date of publication, which was frequently a year later.

Report [on tidal observations on the south shore of Massachusetts and in Nantucket and Vineyard sounds]. Rpt. 1854, app. 29, pp. 35–37.

Description of tide-gauge used at stations on the open seacoast and in situations exposed to strong currents. Rpt. 1854, app. 53, pp. 190–191.

Report on tidal observations in Nantucket sound. Rpt. 1855, app. 33, pp. 222–223.

Report on the progress of the discussion of the interference tides of Nantucket and Martha's Vineyard sounds. Rpt. 1856, app. 37, pp. 261–263.

Report on the investigation of the tides and tidal currents of New York harbor and its dependencies and at Sandy Hook. Rpt. 1856, app. 39, pp. 264–266.

Report of observations of tides and currents in Nantucket and Martha's Vineyard sounds and in the East River at Hell Gate with remarks on the revision of levellings on Hudson River. Rpt. 1857, app. 35, pp. 350–354.

Letter from the Superintendent, communicating the description of a tide gauge . . . for taking observations in deep water. Rpt. 1857, app. 50, pp. 403–404.

Report on the investigation of currents in the East River at Hell Gate and Throg's Neck, the sub-currents of New York bay and harbor and levellings on the banks of the Hudson River. Rpt. 1858, app. 28, pp. 204–207.

Report on the physical survey of New York harbor and the coast of Long Island, with descriptions of apparatus for observing currents. Rpt. 1859, app. 26, pp. 311–317.

Tides and currents in Boston harbor and observations on tides and currents in Cape Cod Bay and Buzzards Bay. Rpt. 1860, pp. 41–43.

Description of implements devised for collecting specimens of bottom in alluvial harbors. Rpt. 1860, app. 39, p. 398.

Report on hydrographic surveys at Oregon inlet and in Neuse River, Military Department of North Carolina. Rpt. 1862, app. 30, pp. 257–259.

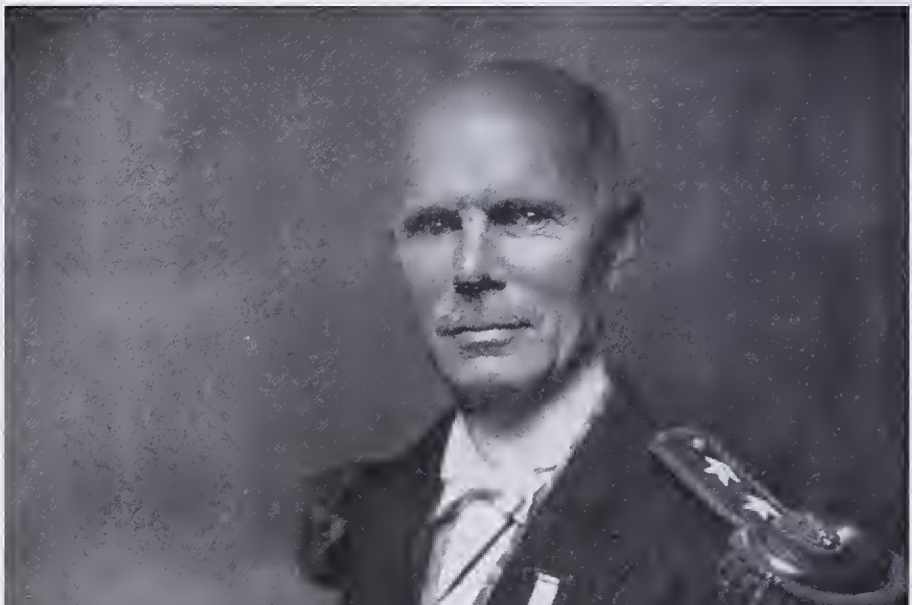
Aids to navigation in Eastport harbor and Penobscot Bay, recommended. Rpt. 1864, app. 23, pp. 223–225.

Report upon soundings across the Straits of Florida. Rpt. 1866, app. 5, pp. 35–44.

Preliminary report on the interference tides of Hell Gate, with directions for reducing the soundings. Rpt. 1866, app. 6, pp. 44–46.

Report to the Superintendent of the Coast Survey on the tides and currents of Hell Gate, N. Y. Rpt. 1867, app. 13, pp. 158–169.
Report upon surveys in the Merrimack River. Rpt. 1867, app. 14, pp. 170–175.
Report on soundings made to develop the character of the Strait of Florida between Key West and Havana. Rpt. 1867, app. 15, pp. 176-179.
Note on Gulf Stream observations. Rpt. 1868, app. 11, pp. 166–167.
Tides and Tidal Phenomena: for the use more particularly of U. S. naval officers. Navy Scientific Papers, No. 2, 1868.
On the reclamation of tide-lands and its relation to navigation. Rpt. 1869, app. 5, pp. 75–104.
Report concerning Vineyard Haven: its character as a port of refuge and its present condition. Rpt. 1869, app. 15, pp. 239-254.
Report on physical surveys made at Martha's Vineyard and Nantucket during the summer of 1871. Rpt. 1869, app. 15, pp. 254–259.
The Coast of Egypt and the Suez Canal. North American Review, Oct. 1869, pp. 475–509.
Extract from a report . . . relative to a method of determining elevations along the course of a tidal river without the aid of a levelling instrument. Rpt. 1870, app. 11, pp. 98–99.
On the probable effect of extended piers in modifying the channel facilities of San Francisco Bay near Yerba Buena Island. Rpt. 1870, app. 18, pp. 180–181.
Report (on the harbor of New York: its condition May 1873). Rpt. 1871, app. 8, pp. 110–133.
Report . . . concerning Nausett beach and the peninsula of Monomoy. Rpt. 1871, app. 9, pp. 134–143.
Hints and suggestions upon the location of harbor-lines. Rpt. 1871, app. 10, pp. 144–153.
The middle-ground shoal, New York Harbor. Rpt. 1872, app. 16, pp. 257–261.
Report on the physical survey of Portland Harbor. Rpt. 1873, app. 8, pp. 94–102.
Additional report concerning the changes in the neighborhood of Chatham and Monomoy. Rpt. 1873, app. 9, pp. 103–107.
Note concerning changes in the submerged contours off Sandy Hook. Rpt. 1873, app. 10, pp. 108–110.
Report on an inspection of the terminal points of the proposed canals through Nicaragua and the Isthmus of Darien. Rpt. 1874, app. 12, pp. 135–147.
Report concerning recent observations at South pass bar, Mississippi River. Rpt. 1875, app. 11, pp. 189–193.
Changes in the harbor of Plymouth, Mass. Rpt. 1876, app. 9, pp. 143–146.
Report upon the physical survey of New York Harbor, 1876. Rpt. 1876, app. 10, pp. 147–185.

Report concerning the location of a quay or pier line in the vicinity of the United States Navy-Yard at New York. Rpt. 1876, app. 11, pp. 186–189.
Characteristics of South pass, Mississippi River. Rpt. 1876, app. 12, pp. 190–191.
Notes concerning alleged changes in the relative elevations of land and sea. Rpt. 1877, app. 8, pp. 98–103.
On a physical survey of the Delaware River in front of Philadelphia. Rpt. 1878, app. 9, pp. 121–173.
Physical hydrography of the Gulf of Maine. Rpt. 1879, app. 10, pp. 175–190.
Addendum to a report on a physical survey of the Delaware River. Rpt. 1879, app. 13, pp. 199–200.
Report on a new rule for currents in Delaware Bay and River. Rpt. 1881, app. 18, pp. 464–469.
Study of the effect of river bends in the lower Mississippi. Rpt. 1882, app. 16, pp. 433–436.
The estuary of the Delaware. Rpt. 1883, app. 8, pp. 239-245.
A plea for a light on St. Georges Bank. Rpt. 1885, app. 11, pp. 483–485.
A report on Monomoy and its shoals. Rpt. 1886, app. 8, pp. 255–260.
A report on the delta of the Delaware. Rpt. 1886, app. 10, pp. 267–279.
On the circulation of the sea through New York Harbor. Rpt. 1886, app. 13, pp. 409–432.
On the movements of the sands at the eastern entrance to Vineyard Sound. Rpt. 1887, app. 6, pp. 159–163.
Addendum to a report on the estuary of the Delaware. Rpt. 1887, app. 13, pp. 269–273.
Report on the results of the physical surveys of New York Harbor. Rpt. 1887, app. 15, pp. 301–311.
Circulation of the Sea through New York Harbor. Science, Vol. 9 (1887) pp. 213–214.
Viscount Ferdinand de Lesseps [Biographical notice]. Proc. Amer. Acad. Arts and Sci. New Series Vol. 23 (1896) pp. 370–384.



George O. Squier

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BIOGRAPHICAL MEMOIR

OF

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PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

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George Owen Squier was a remarkable combination of American soldier, applied scientist, inventor, and engineer, as well as an army administrator and an outstanding Chief Signal Officer.

He was born at Dryden, Michigan, March 21, 1865. His parents were Almon Justice and Emily Gardner Squier. He entered West Point Military Academy at the age of eighteen, and passed through his four years cadetship training with distinction, graduating in 1887. In after years he used to say that at one time in his West Point career he accidentally fell half an hour behind in the routine of his studies, and that it took all his efforts during the remainder of his course to catch up with the schedule. He held the West Point course in high esteem and regarded the incident as an index of its precision. He often told interesting anecdotes of West Point cadet life, illustrating the *esprit de corps* which the institution develops among United States Army officers.

On graduating from West Point in 1887 Squier was appointed a Second Lieutenant in the 3rd Artillery Corps. His training in that branch of the service showed him the importance of accurate scientific knowledge in ballistics and ordnance engineering. He, therefore, took up the study of those subjects, by entering Johns Hopkins University at Baltimore as a graduate student, specializing in mathematics, physics, and ballistics. He became a fellow of Johns Hopkins in the academic year 1891-1892, and received there his Ph.D. degree in 1893, his graduating thesis being on the subject of chemical effects due to magnetism.

He was then appointed a First Lieutenant in the 3rd Artillery Corps and an ordnance instructor at the United States Artillery School in Fortress Monroe, Virginia. At this school he developed instruments for measuring the recoil of guns in action and the velocities of their projectiles. These researches were embodied in several papers and in a book written jointly with Dr. Albert Cushing Crehore—"The Polarizing Photo-chronograph".

At the outbreak of the War with Spain in 1898 Dr. Squier

sought service in the Signal Officer Volunteers and entered with the grade of Captain. In this service he was sent to the Philippine Archipelago, in 1900, where he commanded the cable ship *"Burnside,"* and laid a system of submarine cables between strategic points in the islands. He rightly concluded that the number of infantry men required to maintain effective garrison control over the islands could be greatly reduced by an extensive network of cable and wire communication, terminating in army headquarters. After the war he was appointed to the United States Signal Corps, first as Captain, later as Major, and finally as Chief Signal Officer in the California district.

It was during this period that he took up the study of army cable and radio communication, publishing several papers in this field. Major Squier discovered that a growing tree could serve as a receiving radio antenna if a nail were driven into it fairly high up and a wire brought down from the nail to the receiving instrument on the ground. As a corollary to the proposition that trees and their branches have sufficient conductance to serve as radio antennas, he showed that forests, shrubs, and vegetation generally act as partially absorbent media for radio waves passing over forest land areas.

He also made a study of aviation, then in its early stages of development. In 1908, Major Squier was the first passenger taken up into the air by the aviation pioneer, Orville Wright, in the latter's early form of airplane at Fort Myer. Twenty years later, the two men met in Washington to compare their aviation experiences; the first passenger thus comparing notes with the world's first aviator.

From the earliest days of the Wright brothers' flying machine Squier recognized the immense military importance of the airplane. A large part of his work as Chief Signal Officer was directed toward improving the range, power and effectiveness of this arm of the service as a separate branch of the military art. He succeeded in bringing the American military airplane into the front line of effectiveness during the World War. He foresaw that the bombing airplane would become a mighty engine of destruction in future wars.

In 1911 Squier was granted several United States patents for

transmitting telephone messages over cabled telephone conductors, using high frequency alternating current generation and the modulation of this impressed inaudible tone through the use of a microphone transmitter. This carrier frequency principle has since proved of great service in both wire and wireless telephony. Squier gave to it the name of *"wired-wireless"*. This invention added greatly to his fame as a scientist and engineer. General Squier also contributed a number of inventions for military service, notably a *"quick-aid" kit* for Army and Red Cross first-aid work.

From 1912 to 1916, Lieutenant Colonel Squier was a military attaché to the United States Embassy at London, where he made a special study of European military aviation and where the British Army authorities gave him special facilities for investigation. He was a close observer of the British technical radio and aviation activities during the first two years of the World War. He furnished an extensive report to the United States War Department of these activities. The United States Ambassador to Great Britain at that time, Walter H. Page, wrote a glowing account in his memoirs of Colonel Squier's services in London. Recalled to America in May, 1916, Squier was put in charge of the United States Signal Service as Chief Signal Officer. He organized and administered the electrical communication service between the American Expeditionary Force in Europe and its base in America, using for that purpose electrical communication of all types by radio, cable, and wires. This service continued until two years after the war. He was raised to the rank of Brigadier General in 1917, and from May 20, 1916, to May 20, 1918, was in charge of the Army Air Service, later receiving the distinguished service medal (D. S. M.) for his services.

General Squier was technical adviser to the American delegation at the International Conference on Electrical Communications in Washington during 1920. In 1921 he represented the State Department at sessions of the International Conference on Electrical Communications in Paris, and in the same year was an expert assistant to the American delegation at the Conference on Limitation of Armament, held in Washington.

General Squier was notable for his swiftness of judgment, resolute courage, and earnestness of purpose. His wiry, erect bearing and purposeful demeanor marked him at once as a military officer. He was ever punctual and precise in all engagements, while cheerfully putting late arrivals at their ease. He used to say that one of the many gifts of radio to the world was the radio announcer's habit of broadcast punctuality, even to the extent of ruthlessly cutting off a broadcast in the middle of a word. General Squier was a hard worker, facing every task with cheerfulness and courage. He never married, but was a family friend in numerous homes. With the aid of his sister, Mrs. Mary Squier Parker, who survived him, he built a "country club for country people" at his birthplace, Dryden, Michigan, where he succeeded in giving summer country associations to many of his friends and fellow townspeople. The Club has daily drawn hundreds of persons during the summer months, for recreation in games, boating, swimming, and other sports. This Club was one of General Squier's favorite hobbies. After his retirement from the Army in 1924, he frequently spent his winters in Florida and the other seasons in Washington and Dryden. Wherever he went, General Squier brought brightness and enjoyed popularity. His staff was enthusiastic in its praise and esteem for him.

Numerous honors were bestowed on General Squier both in this country and abroad. He was a Commander of the French Legion of Honor, a Knight Commander of St. Michael and St. George in Great Britain, a Commander of the Order of the Crown of Italy, and a member of the Royal Institution of Great Britain. General Squier held membership in the National Academy of Sciences, the American Philosophical Society, and was a fellow of Johns Hopkins University. He also received an honorary degree from Dartmouth College. General Squier was a life member and fellow of the American Institute of Electrical Engineers. He received from the Franklin Institute, the John Scott Medal in 1896, the Elliott Cresson Medal in 1912, and the Franklin Medal in 1919.

He died at Washington, March 24, 1934, at the age of 69.

PATENTS

Patent no. 620,746, March 7, 1899. Means for impressing semicycles of electromotive force on electric circuits. (With A. C. Crehore.)

Patent no. 625,647, May 23, 1899. Alternating current range and position finder. (With A. C. Crehore.)

Patent no. 655,203, August 7, 1900. Telegraphic transmitter and receiver. (With A. C. Crehore.)

Patent no. 669,149, March 5, 1901. Capillary electrometer for relay purposes. (With A. C. Crehore.)

Patent no. 698,260, April 22, 1902. Transmitting intelligence. (With A. C. Crehore.)

Patent no. 698,261, April 22, 1902. Submarine-cable telegraphy. (With A. C. Crehore.)

Patent no. 711,941, October 28, 1902. Submarine-cable telegraph. (With A. C. Crehore.)

Patent no. 782,181, February 7, 1905. Wireless telegraphy.

Patent no. 823,206, June 12, 1906. Telegraph transmitter. (With A. C. Crehore.)

Patent no. 851,071, April 23, 1907. Automatic telegraphy. (With A. C. Crehore.)

Patent no. 980,356, January 3, 1911. Multiplex telephony and telegraphy.

Patent no. 980,357, January 3, 1911. Multiplex telephony and telegraphy.

Patent no. 980,358, January 3, 1911. Multiplex telephony and telegraphy.

Patent no. 980,359, January 3, 1911. Multiplex telephony and telegraphy.

Patent no. 1,044,502, November 19, 1912. Method and apparatus for detecting and observing forces. (With A. C. Crehore.)

Patent no. 1,233,519, July 17, 1917. Telegraphy. Mayfair, London, England.

Patent no. 1,281,684, October 15, 1918. Electric signalling. (With L. W. Austin and L. Cohen.)

Patent no. 1,289,668, December 31, 1918. System of electric signalling. L. Cohen, Inventor, assigned one-half to Geo. O. Squier.

Patent no. 844,314, February 19, 1907. Apparatus for regenerating electrolytes.

Patent no. 855,221, May 28, 1907. Electrolytic diaphragm and making same.

Patent no. 939,966, November 16, 1909. Method of molding articles.

Patent no. 941,605, November 30, 1909. Packing material.

Patent no. 942,699, December 7, 1909. Method of making insoluble products of phenol and formaldehyde.

Patent no. 942,700, December 7, 1909. Condensation product of phenol and formaldehyde and method of making the same.

Patent no. 942,808, December 7, 1909. Abrasive composition and method of making same.

Patent no. 942,809, December 7, 1909. Condensation product and method of making same.

Patent no. 942,852, December 7, 1909. Indurated product and method of preparing same.
Patent no. 949,671, February 15, 1910. Method of indurating fibrous and cellular material.
Patent no. 954,666, April 12, 1910. Varnish.
Patent no. 957,137, May 3, 1910. Container for food products.
Patent no. 982,230, January 24, 1911. Coated object and method of making the same.
Patent no. 1,018,385, February 20, 1912. Varnish.
Patent no. 1,019,406, March 5, 1912. Composite cardboard.
Patent no. 1,019,407, March 5, 1912. Method of uniting objects.
Patent no. 1,019,408, March 5, 1912. Wood-finishing.
Patent no. 1,037,719, September 3, 1912. Varnish.
Patent no. 1,038,475, September 10, 1912. Process of making insoluble bodies derived from phenol alcohols.
Patent no. 1,054,265, February 25, 1913. Antifriction device.
Patent no. 1,057,211, March 25, 1913. Impregnating wood and products thereof.
Patent no. 1,057,319, March 25, 1913. Impregnated wood and preparing the same.
Patent no. 1,083,264, December 30, 1913. Sound record.
Patent no. 1,085,100, January 27, 1914. Protective coating composition.
Patent no. 1,088,677, March 3, 1914. Infusible condensation product of metacresol and formaldehyde.
Patent no. 1,088,678, March 3, 1914. Infusible condensation product.
Patent no. 1,133,083, March 23, 1915. Phenolic condensation product.
Patent no. 1,146,045, July 13, 1915. Process for making insoluble bodies derived from phenol alcohols.
Patent no. 1,156,452, October 12, 1915. Insulating composition.
Patent no. 1,160,362, November 16, 1915. Method of making a plastic body.
Patent no. 1,160,363, November 16, 1915. Waterproof object or article.
Patent no. 1,160,364, November 16, 1915. Machine element.
Patent no. 1,160,365, November 16, 1915. Paper-like product and method of making the same.
Patent no. 1,187,229, June 13, 1916. Process of making condensation products of phenols and formaldehyde.
Patent no. 1,187,230, June 13, 1916. Reaction product of hexamethylenetetramin and phenolic bodies and method of making same.
Patent no. 1,187,231, June 13, 1916. Phenolic condensation product and method of making the same.
Patent no. 1,187,232, June 13, 1916. Process for making insoluble bodies derived from phenol alcohols.
Patent no. 1,190,845, July 11, 1916. Process for organic synthesis.
Patent no. 1,200,692, October 10, 1916. Hard-rubber composition.
Patent no. 1,213,144, January 23, 1917. Organic insulating composition.

Patent no. 1,213,726, January 23, 1917. Impregnated coil and method of making the same.

Patent no. 1,216,265, February 20, 1917. Insulating composition.

Patent no. 1,216,266, February 20, 1917. Insulating composition.

Patent no. 1,217,115, February 20, 1917. Phenolic condensation product and process of making same. (With Gotthelf.)

Patent no. 1,233,298, July 17, 1917. Matrix.

Patent no. 1,259,472, March 19, 1918. Composition containing phenolic condensation products.

Patent no. 1,259,473, March 19, 1918. Process of molding articles.

BIBLIOGRAPHY OF GEORGE OWEN SQUIER

Electro-chemical effects due to magnetism. Thesis (Ph.D.) Johns Hopkins Univ.; Amer. Journ. Sci., vol. 45, June, 1893.

(With Albert Cushing Crehore.) Note on a photographic method of determining the complete motion of a gun during recoil. Journ. U. S. Artillery, vol. 4, no. 4, Fort Monroe, Va., Artillery School Press, 1895, pp. 7, pls. 2, diagrs.

(With Albert Cushing Crehore.) Experiments with a new polarizing photo-chronograph, applied to the measurement of the velocity of projectiles. Journ. U. S. Artillery, vol. 4, no. 3, Fort Monroe, Va., Artillery School Press, 1896, pp. 409-452, pls. 14, diagrs. Afterwards included in the authors' work "The Polarizing Photo-chronograph," New York, 1897.

(With Albert Cushing Crehore.) The polarizing photo-chronograph. Being an account of experiments at the U. S. Artillery School, Fort Monroe, Va., in developing this instrument. Originally appeared in Journ. U. S. Artillery, vol. 4, no. 3. New York, J. Wiley & Sons; London, Chapman and Hall, Ltd., 1897, pp. 150, ill., diagrs.

(With Albert Cushing Crehore.) Tests of the synchronograph on the telegraph lines of the British Government. A report to the Postmaster General of the United States. Philadelphia, 1898, pp. 34, ills., diagrs. Printed from advance sheets Journ. Franklin Inst., vol. 145.

Elementary principles of the alternating current. Compiled as guide to the lectures upon this subject to the class of student officers, U. S. Artillery School, Fort Monroe, Va. Artillery School Press, 1898, pp. 26, diagrs.

American Pacific cable. Independent, vol. 52, Feb. 8, 1900, pp. 359-362, map. Sci. Amer. Suppl., vol. 49, Feb. 17, 1900, pp. 20186-20188; Feb. 24, 1900, pp. 20195-20197.

Influence of submarine cables upon military naval supremacy. Sci. Amer. Suppl., vol. 51, Apr. 20, 1901, pp. 21156-21158.

"Team-work" in war. Abstract of a lecture delivered at the U. S. Signal School, Fort Leavenworth, Kansas. U. S. Cavalry Journ., July, 1906.

Field equipment of signal troops. Infantry and Cavalry School, Leavenworth, Kansas, 1907.

The present status of military aeronautics. Appendix I. U. S. Signal Corps specification for heavier-than-air flying machines. 2. U. S. Signal Corps specification for dirigible balloon. 3. Bibliography. Journ. Amer. Soc. Mech. Eng., vol. 30, 1908, pp. 1571-1642. Reprinted (abridged by author) in Annual Report of the Smithsonian Institution, 1908, pp. 117-144, ill.

Advantages of aerial craft in military warfare. Aeronautics, vol. 2, 1908, pp. 17-18.

What mechanical flight means to the army. Harper's Weekly, vol. 52, Nov. 21, 1908, pp. 11-12.

Aerial locomotion in warfare. The military possibilities of the aeroplane and airship. Sci. Amer. Suppl., vol. 67, Jan. 2, 1909, p. 7.

Recent progress in aeronautics. Science, vol. 29, Feb. 19, 1909, pp. 281-289.

Ships in the air and water. Sci. Amer. Suppl., vol. 67, Feb. 6, 1909, p. 83.

(With Albert C. Crehore.) Note on oscillatory interference bands and some practical applications. Reprint No. 150, Bull., Bur. Standards, vol. 7, no. 1, Sept. 1, 1910, pp. 12.

Multiplex telephony and telegraphy by means of electric waves guided by wires. Prof. Paper, Signal Corps, U. S. Army, War Dept. Doc. 390, 1911, pp. 102, ill., diagrs. Reprinted Jan. 1919.

(With Albert Cushing Crehore.) Waves of pressure in the atmosphere recorded by an interferometer barograph. Washington, Apr. 18, 1911, pp. 6, ill.

Multiplex telephony and telegraphy by means of electric waves guided by wires. A paper presented at the twenty-eighth annual convention of the American Institute of Electrical Engineers. Proc. Amer. Inst. Elec. Engrs., May, 1911, pp. 857-905. Reprinted in the Annual Report of the Smithsonian Institution, 1911, pp. 133-153, ill.

Multiplex telephony and telegraphy. Sci. Amer. Suppl., vol. 72, 1911, pp. 4-6, 20-22, 36-38, 52-53.

Important advance in telegraphy. Cable signaling system of G. O. Squier. Literary Digest, vol. 52, Apr. 29, 1916, pp. 1216-1217, portrait.

Electric waves directed by wires. Sci. Amer. Suppl., vol. 72, Dec. 16, 1911, pp. 398-399.

Multiplex telephony. Outlook, vol. 97, Feb. 11, 1911, pp. 294-295.

Aeronautics in the United States at the signing of the armistice, November 11, 1918. An address before the American Institute of Electrical Engineers, New York, 1919. Presented at the 345th meeting, American Institute of Electrical Engineers, New York, Jan. 10, 1919.

Unification of communication engineering. Sci. Amer., vol. 127, Aug. 1922, p. 85, portrait.

The easy course in home radio. Edited by Major General George O.

Squier. New York, 1922, M. H. Ray and The Review of Reviews Co., 7 vols., ill. (incl. portraits), diagrs.

The easy course in home radio. Edited and approved by Major General George O. Squier. New York, 1924.

Multiplex telegraphy and telephony. Letter from the Secretary of Commerce submitting in response to Senate Res. 149, a report concerning the discovery of multiplex telegraphy and telephony and the extent to which this system is now used. Senate Doc. 172, 69th Congress, 2d Sess., 1926, pp. 39.

Telling the world. Baltimore, 1933. The Williams and Wilkins Co. in cooperation with The Century of Progress Exposition, 1933. A Century of Progress Series, pp. 156-162, portrait.

Geo. C. Comstock

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1855–1934

BY JOEL STEBBINS

George Cary Comstock was born in Madison, Wisconsin, February 12, 1855, son of Charles Henry and Mercy (Bronson) Comstock. On his mother's side (Doan) his ancestry is traced to the Mayflower; on his father's side he was descended from William Comstock, who settled in Mystic, Connecticut, in 1628. William came from the town of Culmstock, on the river Culm, not far from Exeter, England. His grandfather moved from New England to Norwalk, Ohio, in 1810, and his father was a resident of Madison when the future astronomer was born. There were four children, three boys and a girl, of whom George was the oldest. One of the boys died in infancy; Caroline lived until about 1915; and Louis in 1938 is the Chairman of the Board of the New York Title Insurance Company, New York City. For business reasons the family had moved from Madison to Kenosha, Wisconsin, then to Sandusky, Ohio, and in 1869 they were living in Adrian, Michigan.

In the fall of that year George entered the high school, pursuing what was then known as the Latin-Scientific course. His natural tastes led him, with no advice from others, to select as far as rigid school curricula permitted, mathematics and physics. Quite unbeknown to his parents he was beginning to cherish an ambition to go to Annapolis. His school work was of such high quality that the superintendent of the school encouraged this ambition and brought him into contact with the Congressman of the district, who had the power of appointment on the basis of a competitive examination. In due course, toward the end of his senior year in high school, the examinations for appointment to the Naval Academy were held, and much was the surprise and satisfaction of the family when they learned that George's name headed the list. This was the first news they had had that George had even contemplated taking the examinations.

The appointment came in a few days, but his mother, having gotten over the first flush of joy and excitement, began to won-

der if after all the Navy was a good place for her son. The Civil War at that time had not receded very far into the background, and memories of the boys lost in the war were still green. After pondering over the matter, with some vigorous family discussions, she finally persuaded George to surrender the appointment upon her promise, backed by his father's assent, to give him four years at the University of Michigan. This decision rendered it necessary for the family to move to Ann Arbor so that George might live at home during his college days. The change to Ann Arbor was accomplished just before the beginning of the college year in 1873.

He matriculated that fall and became a candidate for the degree of Ph. B. Almost immediately he became acquainted somewhat as a friend and associate with two professors of mathematics, W. W. Beman and Edward Olney. George's classroom work caused him no worry or anxiety and he was privately consorting with the professors in discussions of advanced mathematics in the evenings. It was during one of these evening meetings with the staff that he was permitted to meet James C. Watson, Professor of Astronomy and Director of the Observatory. Watson at once took a fancy to Comstock, presumably because of his mathematical precocity, and it was Watson who suggested to him the study of astronomy. Into this new field Comstock entered with ardor and zeal.

It was in 1854 that the German astronomer, Francis Brünnow, had been called to Michigan. Trained in the traditions of his home institutions, Brünnow carried to a midwestern college the methods of a German university, and lectured in broken English to despairing and diminishing classes until Watson was his only student. Yet there was developed by Watson, who ultimately succeeded Brünnow, and the others at Michigan, the leading school for the study of astronomy in the country at that time. Of the astronomical graduates during or shortly before Comstock's time may be mentioned R. S. Woodward, Otto Klotz, C. L. Doolittle, and J. M. Schaeberle, and within about the decade following, W. W. Campbell, A. O. Leuschner, and W. J. Hussey. Comstock afterwards referred to Watson as the

cleverest astronomer he had ever met, but one who unfortunately had distributed his energies over too many fields.

Toward the end of his freshman year, following the panic of 1873, George saw the necessity of earning some money during the summers. Watson knew of the U. S. Lake Survey, then in progress on the Great Lakes under General Marr of the U. S. Army. Through Watson's influence George went to see General Marr with whom he concluded an arrangement to enter the service of the Survey, then in the Department of War. By the terms of this arrangement George spent six months in the summer, with leave for six months during the winter at the university. From then on his college course was shortened to six months each year, while he practically lived out of doors in a regular army camp for the remainder of the time. He took part as recorder and assistant engineer in the survey of Lakes Ontario, Erie, and Superior. His last year was on the upper part of the Mississippi River. In this work he became very expert in the use of the theodolite and level, an experience which was later to bring forth his text-book on field astronomy for engineers. During this same interval R. S. Woodward was also connected with the Lake Survey, but there is no record of the paths of the two young scientists crossing at this time.

George was graduated from the University of Michigan in 1877, and after an additional year on the Mississippi River and some further work at the observatory at Ann Arbor in connection with Schaeberle, he followed Watson to the University of Wisconsin late in 1879, to be assistant to the newly founded Washburn Observatory of which Watson was the first director. The scientific work of this observatory was scarcely started when Watson's premature death occurred in 1880, in only the second year of his residence at Madison. Edward S. Holden, later to become the first director of the Lick Observatory, took charge of the Washburn Observatory in 1881, and Comstock continued as assistant. During this period under Holden we find the first work of Comstock's in the Publications of the Washburn Observatory. Among the titles are: "A Catalogue of 195 Stars for 1880"; "A Table of Precessions in Right Ascension and Declination for 1880"; "On a New Method of Observing with the

Prime-Vertical Transit"; "Reduction of Observations Made By Two Observers for the Determination of the Latitude of Washburn Observatory by the Zenith Telescope"; "Determination of the Latitude of the Washburn Observatory by Transits of Stars over the Prime Vertical". It is seen that his activities were all in the astronomy of precision. Later under Holden's direction he did most of the work of preparing the "Tables for the Lick Observatory", which appeared in Volume I of the Lick Publications and have long been used at that institution.

Although Comstock was developing rapidly in his professional work, a career in astronomy involved considerable uncertainty, and he devoted his spare time to the study of law. He was graduated from the Wisconsin law school in 1883, and was admitted to the bar but he never practiced. Nevertheless, he later often referred to his legal training as possibly the most valuable part of his education. His precision of speech and his orderly habits were no doubt accentuated during his law studies.

At the age of thirty he was definitely committed to an academic career by an opening at Ohio State University, where he served as professor of mathematics for two years. He spent the summer of 1886 at the Lick Observatory where it was planned that he would take a position on the staff, but in 1887, when Holden left to take up active service at Mount Hamilton, it was President T. C. Chamberlin who called Comstock to take charge of the Washburn Observatory.

Throughout his scientific activity Comstock held an unusually happy balance between theory and practice. Though the observational astronomy of his early days consisted essentially of the visual measurement of angles, he never became a routine observer. The first work which he took up on assuming the directorship was novel in conception. As a substitute for the meridian circle and clock he placed a prism with reflecting surfaces in front of a telescope, and by observing simultaneously pairs of stars separated by arcs of approximately 120° the measures could be carried around the sphere in three steps, with the advantage that the quantities measured were small angles rather than large ones. From this work there resulted one of the best determinations of the constant of aberration made up to that time. Comstock's value

for this constant, 20″.44, differed from the commonly accepted value of Struve by less than its own probable error.

The telescope used in the observations for aberration was Burnham's famous 6-inch refractor, which had been acquired by the Washburn Observatory in the early eighties. This instrument had been taken by Holden to Caroline Island in the South Pacific for the eclipse in 1883, and it was later used by Flint at the 1900 eclipse in North Carolina. The old wooden tube and mounting are in the museum of the Adler Planetarium in Chicago, but the objective with a new mounting is in current use at Madison.

A striking confirmation of the precision of Comstock's work was furnished some forty years later by Mr. Harry Raymond of the Dudley Observatory. In a discussion of star places for the Boss General Catalogue, Raymond found that Comstock's measures in the early nineties gave an excellent set of corrections to the system of star places then available. Expressed in equations these corrections took the form

$$\Delta\alpha_{\alpha} = -0^{s}.0048 \sin\alpha + 0^{s}.0063 \cos\alpha - 0^{s}.0015 \sin 2\alpha + 0^{s}.0048 \cos 2\alpha \quad \text{(Comstock)}$$

$$\Delta\alpha_{\alpha} = -0^{s}.0056 \sin\alpha + 0^{s}.0069 \cos\alpha + 0^{s}.0003 \sin 2\alpha + 0^{s}.0027 \cos 2\alpha \quad \text{(Raymond)}$$

Considering that Comstock's result was only a by-product of other work, the agreement of the respective terms of the two formulae is truly remarkable. Thus we have a modern appraisal of Comstock's skill, ingenuity, and precision.

Involved in the work on aberration was a determination of the atmospheric refraction, which decreases the apparent arc between any two stars in the sky. His measures established the effect of the relative humidity of the air upon the refraction and confirmed the superiority of the Pulkowa tables over the older ones of Bessel. His interest in the refraction was long continued and his retiring presidential address before the American Astronomical Society was entitled "The Atmospheric Refraction". This address was delivered in 1928, nearly forty years after his first published paper on the subject. One of his contentions was that

the effect of the air at low altitudes is not as uncertain as has often been supposed, and that other sources of error have been wrongly attributed to irregularities in the refraction. His simplified formula for the refraction,

$$R = \frac{983\,b}{460 + t} \tan z,$$

where R is the refraction in seconds of arc, b the height of the barometer in inches, and t the temperature in degrees Fahrenheit, gives the result within one or two seconds except under extreme conditions, an approximation sufficiently close for many kinds of work. This simplification of a complicated formula down to its lowest terms was typical of many of his contributions to practical astronomy. Additional terms and constants were devised for cases where greater accuracy was needed, but Comstock's formula for refraction will be remembered and used in its simplest form.

Of miscellaneous investigations extending over the years may be mentioned observations of minor planets and comets, discussion of the variation of latitude, occultations, especially during eclipses of the moon, physical observations of Mars, and a long series of micrometrical observations of Eros for the solar parallax during the opposition of 1900.

Concurrently with other investigations Comstock carried on measures of double stars with the 15-inch refractor for more than thirty years, from 1887, when he took over the directorship, to 1919 when he stopped definitely and collected all measures in a publication of the observatory. His observations were always of the highest quality, exemplifying the truth of the statement that "the precision of a double-star measure bears no direct relation to the size of the telescope with which it is made". He followed a number of interesting binaries and devised new methods of determining their orbits. His vice-presidential address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science in 1894 was on "Binary Stars". In fact, his interest in double stars was continuous throughout his active career.

Typical of his originality was his experiment on stellar color. By placing a grating of rods or coarse wires in front of the 15-inch objective, a series of spectral images was formed at the focus which was almost indistinguishable from ordinary stellar images. The measures of the separation of these spectra on either side of the primary image gave a numerical determination of the effective wave-length of the light of the star concerned. Thus astronomical colorimetry was placed on a quantitative basis. It was no doubt this interest in color which led him to point out the effect of differential atmospheric dispersion on measures of parallax when the objects concerned were of different spectra.

In fact, Comstock was continually attaching something different to one end or the other of his telescope. He devised a slat-screen apparatus for the meridian circle which reduced the image of a bright star to a multiple diffraction pattern, and this arrangement was used by Flint for many years in parallax and position observations.

Another new device was a double-image micrometer which was applied to the detection of the lunar atmosphere. Though not applicable to general micrometrical work, this instrument enabled him to observe the components of wide double stars close to the moon's rim. As no displacement was found up to the very instant of occultation of one star, he could set an upper limit to the negligible density of the moon's atmosphere.

A proposal by Comstock, the technical details of which he left to others to carry out, was the determination of radial velocity of stars by means of objective prisms. This and similar proposals by other astronomers have never worked out in practice, but the suggestions made by Comstock showed that he was alive to the problems of the so-called new astronomy.

The chief outcome of the double-star work was the detection of proper motions of faint stars. One high authority on double stars had stated that there was yet to be brought forth any evidence of the proper motion of a really faint star, but Comstock demonstrated that stars as faint as the twelfth magnitude do move enough to be detected. By the remeasurement of faint companions of bright double stars, observed incidentally by the

Struves and others early in the nineteenth century, he found that, when the known orbital and proper motions of the bright stars were allowed for, the remaining discrepancies were due to the motions of the faint stars. This conclusion was confirmed by a determination of the sun's way from the motions of the faint stars alone. In the work on proper motions he had the co-operation of Albert S. Flint, who determined many of the required modern positions of stars with the meridian circle at Madison.

Struve had found that for stars down to the tenth magnitude there was the empirical relation that the product of the magnitude and the proper motion was a constant, and Comstock extended this relation to the stars two magnitudes fainter. Thus he showed that the twelfth magnitude stars were nearer to us than would be inferred from their apparent brightness. He gave two alternatives: either there is an appreciable absorption of light in space or the stars which he studied are intrinsically fainter than the bright ones. The second alternative has turned out to be the correct one, and the great preponderance of stars of low intrinsic luminosity in a given volume of space, which his work foreshadowed, has been amply confirmed in recent years.

It was Comstock's determination of the proximity of faint stars that led him to the bold suggestion that the Milky Way is an absorption effect. We see farthest in the galactic plane where there is least absorption, while the stars appear fewest toward the galactic poles because their light is largely or totally blotted out in space. This speculation of course had to be abandoned, but it should be viewed in relation to what was current opinion in astronomy at the time. Newcomb had estimated the galaxy to be only ten or twenty thousand light-years across, and in the "Kapteyn Universe" the sun was placed not far from the center. The spiral nebulae still belonged to the galaxy; that they could be external systems of millions of stars had been considered and rejected by expert opinion at the beginning of the century.

The investigation of the aberration and refraction gave Comstock immediately a standing in the profession. When that work was published, appreciation came from various quarters,

notably from Loewy at Paris and Sir David Gill at the Cape, who wrote most friendly letters and discussed different possibilities of the new method.

In 1897 Simon Newcomb, owing to the age limit of the Navy, retired from the directorship of the Nautical Almanac office. Two years later this post was to become vacant again, and Newcomb urged Comstock to be a candidate for a professorship of mathematics in the Navy, with the understanding that he would become director of the Nautical Almanac. The receipt of such a letter as the following from the austere Newcomb must necessarily have been much appreciated.

Washington Jan. 5, 1899
1620 P St.

Dear Professor Comstock:

The post of Director of Nautical Almanac will be vacant next December by the retirement of Professor Harkness. It seems to me you are best available man for it.

Are you not willing to become a candidate for Professor in the Navy if you can look forward to the detail I have mentioned?

I hope you will be here at the proposed meeting of the committee on the Astronomical Society in February.

Very respectfully

S. Newcomb

This letter was followed by further correspondence, but Comstock preferred to remain in Madison.

Comstock was elected to the National Academy of Sciences in 1899, the first of the Wisconsin faculty to receive this honor. He was also a member of the American Academy of Arts and Sciences, and a life member of the Astronomische Gesellschaft.

In 1899 the Secretary of the Navy, John D. Long, appointed the first board of visitors to investigate and report on the conditions of the United States Naval Observatory. The board consisted of two members of Congress and three astronomers, Senator William E. Chandler, Representative A. G. Dayton, and Professors Edward C. Pickering, George E. Hale, and George C. Comstock. After thorough investigation and discussion, which included a canvass of opinion from the leading astronomers of the country, the board made a report which created a stir at

the time, but which was naturally not particularly welcomed by the Navy. The principal recommendation was that the astronomical work of what had become the national observatory should be placed under the direction of an astronomer rather than a naval officer. This reform, since repeatedly urged by scientific men of the country, was never carried out. The Navy has always been able to hang on to this fine place for the shore leave of a captain or rear admiral. Comstock was blamed or complimented for a leading share of the report, which unfortunately accomplished very little.

The American Astronomical Society grew out of the conference of astronomers and physicists held in connection with the dedication of the Yerkes Observatory in 1897. Comstock was one of the organizers of the society, and served for ten years as its first secretary. Later he held the office of vice-president, and in 1925 he was called from retirement to serve a term as president. He was always a prominent figure at the meetings, taking a leading part in the discussions, whether on business or on scientific questions. He was an admirable presiding officer, and he once remarked that it was the function of the administration to pick out and develop undiscovered talent among the younger men.

He was the chairman of the committee of the society appointed to coordinate the observations of Halley's comet in 1910. On the initiative of this committee an expedition in charge of Ellerman was sent to Hawaii to attempt the observation of the head of the comet which projected against the sun's disk. The report of the committee appeared in Volume 2 of the society's *Publications.*

It was during Comstock's term as president that the society was incorporated under the laws of the State of Illinois. The informal status of the organization had been repellant to his mind, and moreover it was just as well for the society to be in a position to receive donations without legal difficulties.

Throughout his career Comstock carried on instruction as well as research. The number of his students was never large, but he was known to those who came to him as a master of clear and apt expression. When the present writer was a gradu-

ate student he thought that Comstock was the best teacher he had ever had, a judgment which has changed little over the years. Yet in the ordinary sense Comstock did very little teaching for his advanced students. They went along much on their own until difficulties arose, and then his ability to elucidate obscure points would be shown. A student learned from him through inspiration and by imitation. He was a methodical observer and an expert computer, and one needed only to be around and watch him to get some of the intangibles which make for successful scientific work. An occasional phrase or sentence, such as "I believe in an astronomer's making his own instruments", was worth more than an hour of formal instruction.

Much of his success with students was due to his linguistic ability. For years the precision and elegance of his English were noted in the university community. He also was fluent in German, French, and Italian.

The relations with students naturally brought out several papers covering problems of instruction. He contributed notes on the adjustments of a sextant, on the establishment of a meridian line, on the graphical representation of a comet orbit, and on the motions of comets when far from the sun. He was an expert in time determinations with small instruments, and he showed that the precision attained with a 3-inch broken transit with a reversal of the instrument on each star was comparable with the best results of large meridian circles, a conclusion amply confirmed by modern experience in longitude determinations.

In the course of his teaching he also had occasion to write several text-books. The first appeared in 1890, a little work entitled "An Elementary Treatise upon the Method of Least Squares, with Numerical Examples of its Application". He boldly assumed without proof the fundamental equation of the law of errors, pointing out that after all the real justification of the method is that it agrees with experience. Though this little work is out of print, there is still no better place for the novice to look up the essentials of least squares, and how to proceed in a simple practical case.

The "Text-Book of Astronomy" was written in 1901 for

students of high school or junior college grade, and was accompanied by a manual with numerous suggestions for the teacher. Illustrative exercises with simple apparatus were proposed, as it was known that many teachers without previous training in astronomy were being called upon to give an elementary course in the subject.

For many years all of the students majoring as civil engineers at Wisconsin were required to take the course in practical astronomy. The attitude of the engineering faculty was that they were not so anxious to have the students learn astronomy as they were to have them get the unusual training in observation and computation under Professor Comstock. In his textbook of "Field Astronomy for Engineers", which appeared in 1903 with a second edition in 1908, he combined the sound instruction in tested methods of practical astronomy with new applications to the ordinary engineer's transit in the field. He showed that the determination of time, azimuth, latitude, and longitude with small instruments could be made much more precise than was ordinarily assumed. In 1919, as part of his war service, Comstock's experience in teaching navigation to prospective mariners led him to get out a little work on "The Sumner Line".

To a faculty member with Comstock's qualifications there naturally came many important university duties. He was chosen chairman of the committee of arrangements at the time of the Jubilee, the fiftieth anniversary of the founding of the University of Wisconsin. One of the important measures of the first year of the administration of President Van Hise at the university in 1904 was the definite organization of the graduate school. He selected Comstock to be the head of the school, and placed on him the task of working out the problems of a new division of the university, one that was growing rapidly both in size and in importance. He held this position until 1920, as chairman, director, and dean, showing in it his qualities of quiet efficiency and breadth of view. He received a school without definite organization and with about one hundred and fifty students; he left it fully organized for teaching and for research and with its number nearly quadrupled.

Early in his work in the graduate school Comstock once re-

marked humorously that he was somewhat handicapped in the making of Ph. D's. by his own lack of a doctor's degree. This defect was remedied in due time by the award of the honorary degree of LL.D. by the University of Illinois in June, 1907, and of Sc.D by the University of Michigan a week later on the thirtieth anniversary of his graduation.

The duties of the graduate school naturally interfered with his scientific work, but probably the most important of all his investigations, that on the proper motions of faint stars, was carried on amidst other duties of administration and instruction. On relinquishing the deanship he continued active for two years more before retiring from the observatory in 1922 at the age of sixty-seven. Although he could have served several years longer he decided to retire, and this decision like all others of his career was clean cut and final. He finished and published the researches on which he had been engaged, leaving no loose ends about to bother his successor.

Comstock was very fortunate in his family life. In 1894 he married Esther Cecile Everett of Madison who with their daughter Mary, now Mrs. George Carey, survives. The home on Observatory Hill was long known as a center of hospitality, especially to the graduate students, in whom the dean and his wife took a personal interest. Perhaps the explanation of his wide sympathies and interest in people and in current events was the fact that he left astronomy behind each day when he closed the observatory door. After his retirement from university service, Professor and Mrs. Comstock traveled around the world, renewing friendships with scientific colleagues in many countries; they returned to settle in Beloit, Wisconsin, just around the corner from the great attraction of three grandchildren. Here he spent the last dozen years of his life.

Despite his dignified or even austere manner Comstock had a keen sense of humor, which combined with a promptness of decision made him equal to any occasion. At the time of the appearance of Halley's comet in 1910, when he saw the popular interest that was impending, he arranged with the university authorities to make a small admission charge on some of the nights when the observatory would be open to the public for

viewing the comet. On one of the days during this rather hectic astronomical period he was called to the telephone by an inquiring taxpayer. The question was: "Professor, what are you going to do with the money you are collecting for a view of Halley's comet?" Promptly came the response: "Madam, we are going to get a new tail for Halley's comet." The reply seemed to be entirely satisfactory.

In his youth Comstock had been a serious individual with little aptitude for play or sports, but in mid-life he took up golf on the insistence of his family. In his later years he became an ardent member of the Rotary Club of Beloit, and was made an honorary life member of the organization. He had the pleasure of visiting and afterwards reporting on various Rotary Clubs in Europe. At home he was in constant demand as a speaker before service clubs, his topics ranging from club education programs and popular talks on astronomy to philosophical discussions of a more severe order.

He was fortunate in maintaining his physical and mental vigor up to the end. He gave a public address just two weeks before he died, and at the last he was ill for only a few days, being taken by an embolism following a minor operation. The end came quickly on May 11, 1934, in his eightieth year. As has been aptly said, there is always an old school in a progressive science. Comstock lived to become one of the old school in point of years, but his outlook was always forward. He saw the astronomy of his youth grow into the astrophysics of the present, but his conception of all science was like that of the heavens described in his own text-book, "A universe which is ever becoming something else and is never finished."

BIBLIOGRAPHY

1881

A catalogue of 195 stars for 1880. Publications Washburn Observatory, 1, 39-72.

1883

A table of precessions in right ascension and declination for 1880. Publications Washburn Observatory, **2**, 261-273.

On a new method of observing with the prime vertical transit. Publications Washburn Observatory, **2**, 279-284, 1883. Astronomische Nachrichten, **107**, 325-330, 1884.

1884

Observations of (33) Polyhymnia. Astronomische Nachrichten, **108**, 41-44, 1884; **111**, 251, 1885; **120**, 251, 1888.

Opposition ephemeris of the planet Polyhymnia. Astronomische Nachrichten, **110**, 415-416.

1885

Reduction of observations made by two observers for the determination of the latitude of the Washburn Observatory by the zenith telescope. Publications Washburn Observatory, **3**, 25-28.

Determination of the latitude of the Washburn Observatory by transits of stars over the prime vertical. Publications Washburn Observatory, **3**, 29-40.

Observations of eclipses. Astronomische Nachrichten, **111**, 319, 1885; **119**, 331-332, 1888; **120**, 311, 1889; **148**, 315-316, 1899.

1886

The meridian circle of the Lick Observatory. Sidereal Messenger, **5**, 225-230.

Provisional value of the latitude of the Lick Observatory. Bulletin California Academy of Sciences, **2**, 121-123.

1887

A new mode of determining the constants of aberration and refraction. Sidereal Messenger, **6**, 310-317.

1888

Note on the determination of the constant of aberration. Astronomical Journal, **7**, 157-159.

Examination of some errors possibly affecting measures of distance with the prism apparatus of M. Loewy. Astronomical Journal, **8**, 17-21.

On the adjustment of a sextant. Sidereal Messenger, **7**, 129-132.

Historical note relative to the name of the planet Juewa 139. Sidereal Messenger, **7**, 214.

Memoir of James Craig Watson. 1838-1880. Read before the National Academy of Sciences, April, 1888. Biographical Memoirs, National Academy of Sciences, **3**, 43-57. Sidereal Messenger, **7**, 273-286.

On the value of a revolution of a micrometer screw. Sidereal Messenger, **7**, 343-346.

1889

Observations of Sappho. Astronomical Journal, **8**, 29.

1890

An elementary treatise upon the method of least squares, with numerical examples of its application. Boston: Ginn & Co. Pp. vi+68.

The difference of declination of 43 H Cephei and Br. 95. Astronomical Journal, **9**, 179-180.

Observations of double stars. 1887-1890. Publications Washburn Observatory, **6**, 25-140.

Meteorological observations for 1887-93. Publications Washburn Observatory, **7**, 1-195, 1890, 1894.

Double star observations at the Washburn Observatory. Sidereal Messenger, **9**, 78-80.

A simple approximate formula for the refraction. Sidereal Messenger, **9**, 186.

1891

Variations of latitude observed at the Washburn Observatory. Astronomische Nachrichten, **127**, 97-104.

On the right ascension of ξ Draconis. Astronomical Journal, **11**, 45.

The secular variation of latitudes. American Journal of Science, **42**, 470-482.

The present condition of the latitude problem. Transactions Wisconsin Academy of Sciences, Arts, and Letters, **8**, 229-232.

Mr. Burnham on double stars. Sidereal Messenger, **10**, 277-279.

Defects of sensitive levels. Sidereal Messenger, **10**, 299-300.

On the efficiency of a small instrument. Sidereal Messenger, **10**, 406-409, 1891. Zeitschrift für Instrumentenkunde, **12**, 1892.

Reappearance of Saturn's rings. Sidereal Messenger, **10**, 468.

1892

On the relation of the periodic and secular variations of the latitude. Astronomical Journal, **11**, 92-93.

On the supposed secular variations of latitudes. Astronomical Journal, **11**, 116-119.

Provisional results of a determination of the constant of aberration. Astronomical Journal, **11**, 161-166.

Introduction to results of meridian circle observations. 1889-1890. Publications Washburn Observatory, **8**, 3-65.

Observations of Mars at the Washburn Observatory. Astronomy and Astrophysics, **11**, 679-680.

1893

On the atmospheric refraction at Madison, Wisconsin. Astronomy and Astrophysics, **12**, 769-779.

Observations of the south polar cap of Mars at the opposition of 1892. Astronomical Journal, **13**, 41-43.

1894

Results of an investigation of the aberration and atmospheric refraction of light made with a modified form of the Loewy prism apparatus. Astronomy and Astrophysics, **13**, 329-341.

Binary stars. Address of the vice-president before the Section of Mathematics and Astronomy. Proceedings American Association for the Advancement of Science, **43**, 27-52.

1895

Observations of the total eclipse of 1895, March 10. Astronomical Journal, **15**, 39.

Observations of comet *c*1895. (Perrine.) Astronomical Journal, **15**, 199.

Studies in spherical and practical astronomy. Bulletin University of Wisconsin, Science Series, **1**, 57-107.

A course in astronomy for engineering students. Science, **2**, 502-506.

1896

A simple but accurate expression for the atmospheric refraction. Astronomische Nachrichten, **139**, 135-138.

On certain systematic errors in the right ascensions of the fundamental stars. Astronomical Journal, **16**, 121-123.

Investigation of the aberration and atmospheric refraction. Publications Washburn Observatory, **9**, 1-203.

Observations of double stars. 1892-1896. Publications Washburn Observatory, **10**, 1-77.

Observations of the Leonid meteors of 1896 (with A. S. Flint). Astronomical Journal, **17**, 35-36.

1897

Some problems in the astronomy of precision. Popular Astronomy, **4**, 467-470.

On the application of interference methods to the determination of the effective wave-length of starlight. Astrophysical Journal, **5**, 26-35.

Note on Washburn Observatory. Popular Astronomy, **5**, 221.

Research-work at the Washburn Observatory. Stellar parallax, the lunar atmosphere, the ocular heliometer. Publications American Astronomical Society, **1**, 7-11.

The Washburn Observatory. Publications Astronomical Society of the Pacific, **9**, 31-33.

1898

The day of the week. Science, **8**, 710-711.

Graphical representation of a comet orbit. Popular Astronomy, **6**, 465-468.

Some investigations relating to zenith telescope latitudes. Publications

American Astronomical Society, **1**, 46-47. Astrophysical Journal, **8**, 230-232.

1899

Observations of the planet Eros made at the Washburn Observatory. Astronomical Journal, **19**, 169-170.

Report of the board of visitors to the United States Naval Observatory, October 2, 1899. By William E. Chandler, A. G. Dayton, Edward C. Pickering, George C. Comstock, George E. Hale. Science, **10**, 747-751.

Observations of the Leonid meteors, November 10-16, 1898. Astrophysical Journal, **9**, 15-19.

Some rescarches in stellar color. Publications American Astronomical Society, **1**, 89-90.

1900

Observing Eros at Washburn Observatory. Popular Astronomy, **8**, 521.

1901

A text-book of astronomy. New York: D. Appleton and Co. Pp. viii+391.

Establishing a meridian line. Popular Astronomy, **9**, 246-249.

Correction to Klinkerfues' Theoretische astronomie. Astronomische Nachrichten, **156**, 383.

Note on the reduction of the Eros observations. Astronomical Journal, **21**, 77-79.

On the orbit of ξ Bootis. Astronomical Journal, **21**, 182-183.

On the orbit of η Cassiopeiae. Astronomical Journal, **22**, 65-66.

Observations of the brightness of Nova Persei (with Joel Stebbins). Astrophysical Journal, **13**, 336-337.

Observations of Eros. 1900-1901. Publications Washburn Observatory, **10**, part 2, 1-37.

The determination of double star orbits. Publications American Astronomical Society, 1, 160-161.

Sur la réduction des observations d'Éros. Conférence Astrographique Internationale de Juillet 1900, **7**, 49-61.

Mesures micrométriques d'Éros. Conférence Astrographique Internationale de Juillet 1900, **7**, 29; **8**, 72.

Correction to André's Astronomie stellaire. Science, **13**, 746.

Review of: Astronomischer jahresbericht. By W. F. Wislicenus. Science, **14**, 216-218.

1902

Review of: The stars, a study of the universe. By Simon Newcomb. Science, **15**, 220-222.

The motion of comets when far from the sun. Popular Astronomy, **10**, 69-72.

Life on Mars. Observatory, **25**, 62-63.

Review of: Histoire de l'Observatoire de Paris. Par C. Wolf. Science, **16**, 59-61.

Preliminary announcement with regard to the proper motions of certain faint stars. Publications American Astronomical Society, **1**, 189.

1903

A text-book of field astronomy for engineers. New York: John Wiley and Sons. Pp. x+202.

Stellar color and its effect upon determination of parallax. Astronomische Nachrichten, **160**, 69-72.

Time determinations at the Washburn Observatory. Science, **17**, 109.

The determination of time and latitude from equal altitudes of stars. Popular Astronomy, **11**, 238-239.

The mass of 85 Pegasi. Astrophysical Journal, **17**, 220-223. Publications American Astronomical Society, **1**, 187.

The sun's motion relative to a group of faint stars. Publications American Astronomical Society, **1**, 203-204.

1904

Provisional results of an examination of the proper motions of certain faint stars. Astronomical Journal, **24**, 43-49.

Occultation of α Tauri 1904, March 22. Astronomical Journal, **24**, 64.

Stellar luminosity and the absorption of star light. Astronomical Journal, **24**, 139-143.

Report on the Washburn Observatory, Madison, Wisconsin. Publications Astronomical Society of the Pacific, **16**, 112-115.

1905

Distribution of the stars. Publications American Astronomical Society, **1**, 237-239.

1906

A proposed method for the determination of radial velocities of stars. Astrophysical Journal, **23**, 148-151.

Apex of the solar motion. Publications American Astronomical Society, **1**, 270-271.

The luminosity of the brightest stars. Astrophysical Journal, **23**, 248-254.

The significance of the star ratio. Publications American Astronomical Society, **1**, 280-282.

1907

A determination of the sun's motion relative to the fainter stars. Astronomical Journal, **25**, 119-125.

The luminosity of the fixed stars. Astronomical Journal, **25**, 169-175.

Observations of double stars. 1897-1906. Publications Washburn Observatory, **10**, part 3, 1-106.

1908

A text-book of field astronomy for engineers. Second edition. New York: John Wiley and Sons. Pp. xii+218.

Approximate ephemerides of the fixed stars. Publications American Astronomical Society, **1**, 301.

The luminosity of the brighter lucid stars. Publications American Astronomical Society, **1**, 307.

The motion of 70 Ophiuchi. Astronomische Nachrichten, **178**, 17-24.

Proper motions of faint stars. Publications Washburn Observatory, **12**, part 1, 1-236.

1909

On the nature and possible origin of the Milky Way. Popular Astronomy, **17**, 339-342.

Simon Newcomb. Science, **30**, 357-358.

1910

Atmospheric refraction near the horizon. Popular Astronomy, **18**, 90-92.

Review of: Preliminary general catalogue of 6188 stars for the epoch 1900. By Lewis Boss. Science, **32**, 155-157.

The passage of Halley's comet, May 18, 1910. Popular Astronomy, **18**, 370.

Proper motions of faint stars. Observatory, **33**, 50-54.

Review of: A treatise on spherical astronomy. By Sir Robert Ball. Astrophysical Journal, **31**, 91-92.

The absorption of star light considered with relation to the galaxy. Astrophysical Journal, **31**, 270-273.

On Kapteyn's luminosity curve. Astronomische Nachrichten, **185**, 294-304.

Report of the committee on comets (with E. E. Barnard, E. B. Frost, E. C. Pickering, F. Ellerman). Publications American Astronomical Society, **2**, 42, 105, 144, 177-227, 1910-1915; **3**, 148, 235, 1915-1916.

1911

The proper motions and parallaxes of the tenth magnitude stars. Astronomische Nachrichten, **187**, 113-120.

1912

Proper motions of faint stars. Publications American Astronomical Society, **2**, 125-126.

1913

Proper motions of telescopic stars. Astronomical Journal, **28**, 49-58. Publications American Astronomical Society, **3**, 4-5.

Review of: Measures of proper motion stars made with the 40-inch refractor of the Yerkes Observatory. By S. W. Burnham. Science, **38**, 551-552.

1914

A comparative study of filar micrometer measures of distance. Publications American Astronomical Society, **3**, 78-79.

1916

Review of: Transactions of the International Union for Coöperation in Solar Research. Vol. IV. Science, **43**, 642-643.

1917

The orbit of ζ Herculis. Astronomical Journal, **30**, 139-148.

1918

The orbit of Σ 1879 = B 6999. Astronomical Journal, **31**, 107-111.

Review of: Annals of the Astronomical Observatory of Harvard College, Vols. 79, part 1; 83, part 2; 91. Edward C. Pickering, Director. Science, **48**, 397-398.

1919

The Sumner line or line of position as an aid to navigation. New York: John Wiley and Sons. Pp. vi+70.

Atmospheric refraction near the horizon. Publications American Astronomical Society, **4**, 83-84.

1920

70 Ophiuchi; Σ 2272; B 8340; P. G. C. 4571; and neighboring stars. Astronomical Journal, **32**, 153-159.

Some new methods for double star orbits. Publications American Astronomical Society, **4**, 211-212.

1921

On the determination of double star orbits from incomplete data, first paper, with an application to the orbit of μ^2 Bootis. Astronomical Journal, **33**, 139-145.

On the determination of double star orbits from incomplete data, second paper, with an application to the orbit of γ Coronae Borealis. Astronomical Journal, **33**, 163-168.

Newcomb on extra-mundane life. Science, **54**, 29-30.

Note upon a comparison of proper motions. Astronomical Journal, **33**, 177-178.

Observations of double stars. 1907-1919. Publications Washburn Observatory, **10**, part 4, 1-167.

1922

Observations of proper motion stars. Publications Washburn Observatory, **14**, part 1, 1-187.

A new member of the Taurus cluster. Astronomical Journal, **34**, 33, 60.

1925

Review of: Meteors. By Charles P. Oliver. Astrophysical Journal, **62**, 305-306.

Review of: Probleme der astronomie. Festschrift für Hugo v. Seeliger. Astrophysical Journal, **61**, 204-205.

1926

John Tatlock. Popular Astronomy, **34**, 223-224.

1929

The atmospheric refraction. Retiring address of the president of the American Astronomical Society. Publications American Astronomical Society, **6**, 214-223.

G. P. Clinton

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OF

GEORGE PERKINS CLINTON

1867–1937

BY

CHARLES THOM AND E. M. EAST

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

GEORGE PERKINS CLINTON

Chronology

1867. Born May 7, at Polo, Illinois.
1886. Graduated, Polo High School.
1890. B.S., University of Illinois. Assistant Botanist, Agricultural Experiment Station, and Assistant in Botany in the University of Illinois.
1894. M.S. in Botany.
1900. Graduate Student in Botany, Harvard.
1901. M.S., Harvard.
1902. D.Sc., Harvard.
1902. Botanist, Connecticut Agricultural Experiment Station at New Haven, July 1, 1902.
1902–1925. Botanist, State Board of Agriculture (Connecticut).
1903–1927. Chairman, Committee on Fungous Diseases of Connecticut Pomological Society.
1904. Agent and Expert, Office of Experiment Stations, U. S. Department of Agriculture, studying coffee diseases in Puerto Rico.
1906–1907. Studied fungous diseases of the brown-tail moth at Harvard, 1906. Continued the study in Japan, 1907.
1908. Agent, Bureau of Entomology, U. S. Department of Agriculture, to prevent the spread of moths.
1909. Collaborator, to seek parasites of the Gypsy moth in Japan.
1915–1927. Lecturer in Forest Pathology, Yale.
1916. Collaborator, Plant Disease Survey, U. S. Department of Agriculture.
1926–1929. Research Associate in Botany, Yale.
1937. Died, August 13th at New Haven, Connecticut.

GEORGE PERKINS CLINTON*

1867–1937

BY CHARLES THOM AND E. M. EAST

George Perkins Clinton was born in Polo, Illinois, May 7, 1867. He was known to his fellows as Botanist of the Connecticut Agricultural Experiment Station, a post which he held for thirty-five years. His background was an ancestry which had its roots among the founders of New York and New England. Direct progenitors on both sides of the family served in the American Revolution. He was the son of John Waterbury Clinton who was born in Andes, New York, but moved to Ogle County, Illinois, when a young man, as a teacher; there he married Caroline Perkins whose family had moved from Delhi, New York, to Buffalo Grove, Illinois. Two great-grandfathers, Joseph Clinton and Rufus Perkins, fought in the Revolutionary War; one grandfather, Timothy Perkins, in the War of 1812, and an uncle, Edgar Perkins, was in the Civil War. John Waterbury Clinton published the "Polo Ogle County Press" for nearly fifty years. He was interested in all the early movements toward popular education which resulted in the establishment of colleges throughout the state. As editor of a paper in a community predominantly interested in farming, he supported the development of the University of Illinois, and especially its agricultural work. He was a collector of coins, stamps, geological specimens, personal and historical material especially concerning the region in which he had settled. His house was full of books and magazines. His one hobby that may have turned his son toward botany, was his development of flower gardens. We have thus the background of a scholar.

One of his teachers writes that young Clinton was a "studious, hard-working pupil," "very much disgusted and impatient with himself if he made a mistake" and very ambitious to make a record for scholarship. There is no report of his first stimulus

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* Dr. Florence A. McCormick, who was closely associated with Dr. Clinton during the later years of his work at New Haven, collected most of the materials used in this memoir.

to study plants but by the time he graduated from high school at Polo in 1886, he had acquired the beginnings of a collection of native species of the prairies, and an interest in botanical training which led him to seek information about the University of Illinois. This brought him into contact with M. B. Waite of Oregon, Illinois, not far from his home. Mr. Waite was already specializing in botany at the University and working in the field as assistant during the summer. Clinton entered the University in the fall of 1886.

The great figure in botany at the University of Illinois for many years was Prof. T. J. Burrill, who was about at the height of his career when Clinton was in college. In connection with The Illinois Natural History Survey, Burrill kept his special students and assistants collecting and classifying the flora of the state. Clinton was early assigned to the fungi and when he graduated in 1890, went to work for the University as an assistant. Preliminary publication upon two of the great groups of parasitic fungi had been made by others before Clinton graduated. The manuscript upon the smuts (Ustilagineae) was in process and fell to him. He published the smuts of Illinois while in the service of the University.

He remained with Dr. Burrill until 1900, working for the most part upon the fungi of the state. Fourteen publications are reported for this period. Those of us who knew him in the laboratory in the late 1890's and shared an acquaintance with him for the next forty years, know that he changed little in appearance during that time. Then as later, he was a slight man, serious, methodical, purposeful, chary of word unless he was sure of his audience but worthy of a hearing when he chose to speak.

One who saw much of him during his professional career reports that in spite of close and persistent application to the work on hand, "He was never too busy to help a comrade or a colleague, to the best of his ability; and his comments never failed to show a keen insight into the problem under discussion" and again, in thirty years of acquaintance he was not heard to

say, "A word about an acquaintance that could be called sharp, or mean or disagreeable."

The casual visitor found Clinton the formal, cautious scientist, sparing in the use of words but succinct in his discussion of the problem presented. Only when one got behind the scenes and found him jealously using and treasuring for thirty years the chair and desk inherited from Thaxter, he caught the strain of hero worship which put high value upon the ideas and ideals of his predecessor. Then in his memoir of that friend, we find the poetic spark which fired that elemental imagination which somehow lights the way for every constructive worker through long years of unremitting application to problems which are deadly to men who lack that inspiration.

The correspondence of those ten years in the Illinois Survey inevitably brought him into contact with Dr. Farlow at Harvard. This led to a visit to Cambridge, and in 1900 Clinton took leave from his University assistantship for graduate study with Professors Farlow and Thaxter. He took with him his Smuts of Illinois. His thesis extended it to the Smuts of North America and, like many another who fell under the spell of Thaxter—he carried away that group as a life task. Other assignments might come and go, the monographic work upon the smuts was still in process when he died. Fortunately he had arranged with associates for the completion of work in progress thus to insure that his revision of the Smuts of North America will be published.

He received the doctorate of science at Harvard in 1902, and became a member of the honorary society of Sigma Xi. The post of botanist at the Connecticut Agricultural Experiment Station at New Haven, once honored by Thaxter himself, was given to Clinton, July 1, 1902. He retired from it on July 1, 1937, after thirty-five years of distinguished service, and died August 13, 1937.

The record of those thirty-five years appears principally in the biennial reports of the "Station Botanist" and in the publications of the state societies interested in agriculture. Clinton's survey of the outstanding disease records of a year show how

close a watch he maintained upon the parasitic diseases of Connecticut crops. In addition, each such report shows one or more studies of particular diseases or of special fungi or fungous groups. Connecticut presents a wide range of conditions—forests, rolling hills, areas devoted to forage crops, intensive market gardening, tobacco as a special crop, apples, peaches on a commercial scale, ornamentals, and greenhouse products—one has but to read the topics in Clinton's reports to find that he was keenly alive to the problems of crop production throughout the state. No group was ignored or neglected.

The working plant pathologist consistently watched Clinton's reports for summaries of progress, for new methods, for observations and illustrations of the newly recognized disease or for the foreign invader. In each case the history and significance of the disease was presented together with the definite additional material developed by Clinton and his colleagues working at New Haven. These papers were illustrated by photographs and drawings chosen to convey to the grower as complete as possible a concept of the type of injury to crop plants attributable to the infecting agent. Many fungi were carried to the laboratory where life histories were worked out carefully. Among such special studies, we find Phytophthora of potatoes with special investigation of the production of oospores, Phytophthora of Lima beans, Thielavia as a root rot of tobacco, the bacterial disease—tobacco "wild fire," an elaborate series of papers upon the chestnut bark disease, another series upon white pine blister rust, a survey of the heteroecious rusts observed in the state.

He began work on what are now known as the virus diseases about 1903. In various forms, references to the group appear in each report. In 1914, experimental work on tobacco mosaic over a number of years was brought together in his well known paper—Calico of Tobacco. The whole matter again appears in his joint paper with Dr. McCormick in 1928.

Clinton was an active member of various societies covering the agricultural field in Connecticut. Few state meetings were held without his presence and participation. Aside from his

general reports, his name appears upon eighteen committee reports to the Connecticut Vegetable Growers' Association, upon twenty-four reports to the Connecticut Pomological Society, and as author or joint author of twenty Experiment Station bulletins. He interpreted his place of the Station Botanist as a service job. To the broad interest in plants and consecration to the interests of the state so typical of Burrill, he added the critical taxonomic point of view characteristic of Farlow and the persistent, patient descriptive ideals of Thaxter.

As a Connecticut scientist, in addition to the groups already mentioned, he was a member of the New England Botanical Society, the Connecticut Botanical Society, the Connecticut Forestry Association.

The merit of his contributions to agriculture and to botany was recognized early by his colleagues beyond the state service. Although he was modest and unassuming, he was constantly in demand for counsel and instruction. Harvard called him back once. Yale made use of him for a dozen years. The United States Department of Agriculture requisitioned his service from time to time and the Phytopathological Society made him its president in 1912. His election to the National Academy of Sciences was welcomed by all who knew him in 1930.

In addition, he was a Fellow of the American Association for the Advancement of Science, Fellow of the American Academy of Arts and Sciences since 1914, member of the Mycological Society of America, of the American Society of Plant Physiologists, of the American Naturalists and of the Botanical Society of America.

Clinton was a confirmed collector. He began building his personal herbarium as a boy at Polo, Illinois. Four years at the University added the plants of South Central Illinois; ten years on the Survey made him a discriminating collector whose critical eye caught host and parasite in the same glance. His herbarium grew from every journey and each assignment. Thus he added series from Canada, from the United States, from Puerto Rico, Panama, Hawaii, Japan and from parts of Europe reached on his trips abroad. Each expedition was a scouting trip—his wide

acquaintance with host plants and parasites made him alert to detect the spread of plant disease into new territory and to anticipate its significance to his own state.

His herbarium with the record of fifty years of field work, was given to the Connecticut Agricultural Experiment Station. The descriptive literature assembled by him remains with the collection. The two together will make that station a place of pilgrimage to the phytopathologist seeking to follow the story of the fight to save our crop plants from destructive pests.

Clinton married Anna J. Lightbody of Pekin, Illinois, on August 9, 1892. Their only son, Harry Lightbody Clinton, was killed in France, fighting with the American troops in the World War. This loss saddened the latter years of the father's life. He never lost his sense of acute grief. Work in increasing amounts was his only means of escape. While he retired voluntarily July 1, 1937, he retained the title of consulting botanist and went to work each day as usual, as long as he was able to get there. Mrs. Clinton continues to live in New Haven, Connecticut.

The annual loss to agriculture from plant diseases is variously estimated all the way from half a billion to three billion dollars. Whatever may be the true figure, the loss is great. The only protection of the public against such enemies, is a home guard, smaller than an army regiment in times of peace, composed of fighters armed with that knowledge of the character and habits of the enemy which comes from thorough training in plant pathology. One who occupied an outpost along this battle line was lost when George Perkins Clinton died August 13, 1937.

BIBLIOGRAPHY *

Observations on a fungus (Saprolegnia) infesting the fish. Bull. U. S. Fish. Com. for **1893**: 163–172. 1893.

Orange rust of raspberries and blackberries. U. Illinois Agr. Expt. Sta. Bull. **29**: 273–300. 1893.

Relationship of Caeoma nitens and Puccinia Peckiana. Bot. Gaz. **20**: 116–117. 1895.

The Russian thistle and some plants that are mistaken for it. U. Illinois Agr. Expt. Sta. Bull. **39**: 87–118. 1895.

Fungous diseases of the potato. U. Illinois Agr. Expt. Sta. Bull. **40**: 136–140. 1895.

An experiment to prevent scab and leaf blight of potatoes. U. Illinois Agr. Expt. Sta. Bull. **40**: 140–145. 1895.

Smuts and their prevention. U. Illinois Agr. Expt. Sta. Special Bull. **372a–372d.** 1897.

Broom-corn smut. U. Illinois Agr. Expt. Sta. Bull. **47**: 373–412. 1897.

Reproduction of plants. The Prairie Farmer, Jan. 2 and 9, 1897.

Smut enemies of the Illinois farmer. The Illinois Agriculturist. **II**: 9–18. 1898.

The smuts of Illinois' agricultural plants. U. Illinois Agr. Expt. Sta. Bull. **57**: 289–360. 1900.

Apple scab. U. Illinois Agr. Expt. Sta. Bull. **67**: 109–156. 1901.

Two new smuts of Eriocaulon septangulare. Rhodora **3**: 79–82. 1901.

Apple rots in Illinois. U. Illinois Agr. Expt. Sta. Bull. **69**: 189–224. 1902.

North American Ustilagineae. Jour. Mycol. **8**: 128–156. 1902.

Cladochytrium Alismatis. Bot. Gaz. **33**: 49–61. 1902.

Parasitic fungi. Report Conn. Board Agr. for **1902**: 253–269. 1903.

Spray calendar. Conn. Agr. Expt. Sta. Bull. **142.** 1903 (with W. E. Britton).

Diseases of plants cultivated in Connecticut. Conn. Agr. Expt. Sta. Report for **1903**: 279–370. 1904.

The study of parasitic fungi in the United States. Trans. Mass. Hort. Soc. 1904. Part **I**: 91–106. 1904.

Fungi in this country. Boston Evening Transcript, Feb. 27, 1904.

North American Ustilagineae. Proc. Boston Soc. Nat. Hist. 31, No. **9**: 329–529. 1904.

Economic fungi supplement including species of scientific rather than of economic interest. Numbers C 1–C 100; C 101–C 150. Ustilagineae. Published by A. B. Seymour and F. S. Earle. Edited by G. P. Clinton with the cooperation of W. J. Beal, W. C. Blasdale, *et al.* Cambridge, Mass., 1903–1905.

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* Bibliography prepared by Dr. Florence A. McCormick. As published in Phytopathology, volume 28, number 6.

Diseases of the potato in Connecticut. Report Conn. Board Agr. for **1904**: 129–143. 1905.
Notes on fungous diseases, etc., for 1904. Conn. Agr. Expt. Sta. Report for **1904**: 311–328. 1905.
Downy mildew, or blight, Peronoplasmopara cubensis (B. and C.) Clint., of muskmelons and cucumbers. Conn. Agr. Expt. Sta. Report for **1904**: 329–362. 1905.
Downy mildew, or blight, Phytophthora infestans (Mont.) De By., of potatoes. Conn. Agr. Expt. Sta. Report for **1904**: 363–384. 1905.
The Ustilagineae, or smuts, of Connecticut. Conn. State Geol. and Nat. Hist. Survey Bull. **5**: 1–43. 1905.
Notes on fungous diseases, etc., for 1905. Conn. Agr. Expt. Sta. Report for **1905**: 263–277. 1906.
Downy mildew, Phytophthora phaseoli Thaxt. of lima beans. Conn. Agr. Expt. Sta. Report for **1905**: 278–303. 1906.
Downy mildew, or blight, Phytophthora infestans (Mont.) De By., of Potatoes, **II**. Conn. Agr. Expt. Sta. Report for **1905**: 304–330. 1906.
Ustilaginales. Ustilagineae, Tilletiaceae. N. Amer. Flora **7**: Part. 1, 1–82. 1906.
Diseases of the potato. Report Conn. Board. Agr. for **1906**: 215–227. 1907.
Notes on fungous diseases, etc., for 1906. Conn. Agr. Expt. Sta. Report for **1906**: 307–331. 1907.
Experiments to prevent onion brittle. Conn. Agr. Expt. Sta. Report for **1906**: 332–335. 1907.
Dry rot fungus, Merulius lacrymans (Wulf.) Schum. Conn. Agr. Expt. Sta. Report for **1906**: 336–341. 1907.
Root rot of tobacco, Thielavia basicola (B. and Br.) Zopf. Conn. Agr. Expt. Sta. Report for **1906**: 342–368. 1907.
Peridermium acicolum, the aecial stage of Coleosporium solidaginis. Science, n. s. **25**: 289–290. 1907.
Notes on fungous diseases, etc., for 1907. Conn. Agr. Expt. Sta. Report for **1907–1908**: 339–362. 1908.
Root rot of tobacco, **II**. Conn. Agr. Expt. Sta. Report for **1907–1908**: 363–368. 1908.
Heteroecious rusts of Connecticut having a peridermium for their aecial stage. Conn. Agr. Expt. Sta. Report for **1907–1908**: 369–396. 1908.
Notes on fungous diseases, etc., for 1908. Conn. Agr. Expt. Sta. Report for **1907–1908**: 849–871. 1908.
Peach yellows and so-called yellows. Conn. Agr. Expt. Sta. Report for **1907–1908**: 872–878. 1908.
Chestnut bark disease, Diaporthe parasitica Murr. Conn. Agr. Expt. Sta. Report for **1907–1908**: 879–890. 1908.
Artificial cultures of Phytophthora with special reference to oospores. Conn. Agr. Expt. Sta. Report for **1907–1908**: 891–907. 1908.

Spray calendar. Conn. Agr. Expt. Sta. Bull. **159**. 1908 (with W. E. Britton).
Notes on certain rusts with special reference to their peridermial stages. Science, n. s. **27**: 340. 1908.
Arthur's Uredinales (Coleosporiaceae, Uredinaceae, Aecidiaceae pars). Bot. Gaz. **46**: 467–468. 1908.
Artificial cultures of Phytophthora with special reference to oospores. Science, n. s. **29**: 271–272. 1909.
Tests on summer sprays on apples and peaches in 1910. Conn. Agr. Expt. Sta. Report for **1909–1910**: 583–618. 1910 (with W. E. Britton).
Notes on plant diseases of Connecticut. Conn. Agr. Expt. Sta. Report for **1909–1910**: 713–738. 1910.
Spraying potatoes in dry seasons. Conn. Agr. Expt. Sta. Report for **1909–1910**: 739–752. 1910.
Oospores of potato blight, Phytophthora infestans. Conn. Agr. Expt. Sta. Report for **1909–1910**: 753–774. 1910.
Duggar's Fungous Diseases of Plants. Amer. Jour. Sci. **IV**: 92–93. 1910.
Peach yellows situation. Farm and Home **31**: 9. 1910.
Oospores of potato blight. Science, n. s. **33**: 744–747. 1911.
McAlpine's Smuts of Australia. Mycologia **3**: 163–164. 1911.
Tests of summer sprays on apples, peaches, etc. Conn. Agr. Expt. Report **1911**: 348–406. 1912 (with W. E. Britton).
Some facts and theories concerning chestnut blight. Penn. Chestnut Blight Conf. Report: 75–83. 1912.
Chestnut blight situation in Connecticut. Penn. Chestnut Blight Conf. Report: 154–157. 1912 (with S. N. Spring).
Notes on some heteroecious rusts of Connecticut. Phytopath. **2**: 94. 1912.
Chestnut blight fungus and its allies. Phytopath. **2**: 265–269. 1912.
The relationships of the chestnut blight fungus. Science, n. s. **36**: 907–914. 1912.
Spray Calendar for Connecticut. Report Conn. Board. Agr. for **1912**: 23–30. 1913 (with W. E. Britton).
Spray Calendar for Connecticut. Report Conn. Board Agr. for **1913**: 26–34. 1914 (with W. E. Britton).
Notes on plant diseases of Connecticut. Conn. Agr. Expt. Sta. Report for **1912**: 341–358. 1913.
Chestnut bark disease, Endothia gyrosa var. parasitica (Murr.) Clint. Conn. Agr. Expt. Sta. Report for **1912**: 359–453. 1913.
Potato inspection in Connecticut. The Conn. Farmer and N. E. Farms 3, May 23, 1914.
Notes on plant diseases of Connecticut. Conn. Agr. Expt. Sta. Report for **1914**: 1–29. 1915.

So-called chestnut blight poisoning. Conn. Agr. Exp. Sta. Report for **1914**: 30–42. 1915.

Spray calendar. Conn. Agr. Expt. Sta. Bull. **183**. 1915 (with W. E. Britton).

Spray treatment, etc., for orchards. Conn. Agr. Expt. Sta. Bull. **184**. 1914 (with W. E. Britton).

Potatoes a cash crop for Connecticut. Fungous Pests. Joint Circ. Inf. No. 1 Agr. Stations and Conn. Agr. Coll. 5–8. 1915.

Chlorosis of plants with special reference to calico of tobacco. Conn. Agr. Expt. Sta. Report for **1914**: 357–424. 1915.

Notes on plant diseases of Connecticut. Conn. Agr. Expt. Sta. Report for **1915**: 421–451. 1916.

Diseases of plants caused by nematodes. Conn. Agr. Expt. Sta. Report for **1915**: 452–462. 1916.

Powdery scab of potatoes. Conn. Agr. Expt. Sta. Report for **1915**: 463–469. 1916.

Potato spraying experiments. Third report. Conn. Agr. Expt. Sta. Report for **1915**: 470–487. 1916.

Botany in relation to agriculture. Science, n. s. **43**: 1–13, 171–172. 1916.

Milburn and Bessey's Fungoid Diseases of Farm and Garden Crops. Science, n. s. **43**: 391–392. 1916.

The potato crop in 1915. Rural New Yorker, 361, 414, 435. 1916.

Co-operative potato spraying, 1916. Conn. Agr. Expt. Sta. Report **1916**: 355–364. 1917 (with F. E. Rogers).

Spray Calendar. Conn. Agr. Expt. Sta. Bull. **199**. 1917 (with W. E. Britton).

Robbin's Botany of Crop Plants. Plant World **21**: 104. 1918.

Plant Diseases. The Home Garden Manual. Com. Food Supply, Conn. State Council Defense 13–15. 1918.

Artificial infection of Ribes species and white pine with Cronartium ribicola. Amer. Plant Pest Com. (ed. 2) Bull. **2**: 14–15. 1919.

Co-operative potato spraying in 1917. Conn. Agr. Expt. Sta. Bull. **214**: 411–420. 1919 (with L. F. Harvey).

Fertilizer experiments with potatoes. Conn. Agr. Expt. Sta. Bull. **214**: 421–422. 1919 (with E. H. Jenkins).

Inspection of Phaenoganic herbaria for rusts in Ribes spp. Conn. Agr. Expt. Sta. Bull. **214**: 423–427. 1919.

Infection experiments of Pinus strobus with Cronartium ribicola. Conn. Agr. Expt. Sta. Bull. **214**: 428–459. 1919 (with Florence A. McCormick).

Artificial infection of pines with Cronartium ribicola. Amer. Plant Pest Comm. Bull. **4**: 12. 1919 (with Florence A. McCormick).

Rankin's Manual of Tree Diseases. Woman's Nat. Farm and Gard. Ass'n **6**: 6–7. 1919.

New or unusual plant injuries and diseases found in Connecticut, 1916–1919. Introduction: 397–398. Dry rot fungus, Merulius lacrymans (Wulf.) Schum: 398–400. Moldy unsalted butter: 400–404. Injuries and diseases of plants arranged according to hosts: 404–482. Conn. Agr. Expt. Sta. Bull. **222**: 397–482. 1920.

Spray Calendar. Conn. Agr. Expt. Sta. Bull. **224**. 1920 (with W. E. Britton).

William Gilson Farlow. Phytopath. **10**: 1–8. 1920.

Wild fire of tobacco in Connecticut. Conn. Agr. Expt. Sta. Bull. **239**: 365–423. 1922 (with Florence A. McCormick).

Spray calendar. Conn. Agr. Expt. Sta. Bull. **244**. 1923 (with W. E. Britton).

Will the chestnut trees come back? The N. E. Farms **54**: 1, 9. 1924.

Rust infection of leaves in Petri dishes. Conn. Agr. Expt. Sta. Bull. **260**: 475–501. 1924 (with Florence A. McCormick).

Fungous and non-infectious troubles of ornamental trees. Conn. Agr. Expt. Sta. Bull. **263**: 171–192. 1924.

A. H. R. Buller's Researches on Fungi, Vol. III. Science, n. s. **63**: 571–572. 1925.

Spray Calendar. Conn. Agr. Expt. Sta. Bull. **271**. 1926 (with W. E. Britton).

Tobacco diseases observed in 1925. Conn. Agr. Tob. Sta. Bull. **6**: 66T–73T. 1926. (with P. J. Anderson).

A new disease of willows appears in Connecticut. U. S. Pl. Dis. Rep. **11**: 87. 1927.

Tobacco diseases observed in 1926. Conn. Agr. Tob. Sta. Bull. **8**: 55T–57T. 1927 (with P. J. Anderson).

Fusicladium saliciperdum (leaf rot) on willows. U. S. Pl. Dis. Rep. **12**: 54–55. 1928 (with Florence A. McCormick).

Leaf rot (Fusicladium tremulae) on poplars in New England. U. S. Dept. Agr. Plant Dis. Rep. **12**: 55. 1928.

Willow Scab (Fusicladium saliciperdum). U. S. Pl. Dis. Rep. **12**: 82. 1928.

Tobacco Mosaic. Conn. Agr. Tob. Sta. Bull. **10**: 75T–82T. 1928 (with Florence A. McCormick).

George Richard Lyman (1871–1926). Proc. Amer. Acad. Arts and Sci. 62. No. **9**: 268–273. 1928.

Willow scab blight. Nat. Shade Tree Conf. **5**: 61–62. 1929.

The willow scab fungus, Fusicladium saliciperdum. Conn. Agr. Expt. Sta. Bull. **302**: 443–469. 1929 (with Florence A. McCormick).

Coleosporium solidaginis on red pine in Connecticut. U. S. Dept. Agr. Pl. Dis. Rep. **13**: 110. 1929.

Further notes on willow scab. U. S. Dept. Agr. Pl. Dis. Rep. **13**: 110. 1929.

The willow scab disease. Rep. Blister Rust Control Conf. **14:** 21–22. 1928.

Some effects of drought on shade trees. Proc. Nat. Shade Tree Conf. **7:** 34–36. 1931.

Notes on three serious tree diseases. Blister Rust Control Conf. Rep. **17:** 56–62. 1931. (Mimeo.)

Notes on three serious tree diseases. Quebec Soc. Protect. Plants. Ann. Rep. Nos. **23–24:** 13–19. 1932.

Drought injury and sun scorch. U. S. Dept. Agr. Pl. Dis. Rep. Suppl. **85:** 65. 1933.

Plant Pest Handbook for Connecticut. II. Diseases and injuries. Conn. Agr. Expt. Sta. Bull. **358:** 153–329. 1934.

The Dutch elm disease, Graphium ulmi, in Connecticut. Science, n. s. **81:** 68–70. 1935 (with Florence A. McCormick).

Dutch elm disease, Graphium ulmi. Conn. Agr. Expt. Sta. Bull. **389:** 701–752. 1936 (with Florence A. McCormick).

Erwin Frink Smith (1854–1927). Proc. Amer. Acad. Arts and Sci. **70:** 575–578. 1936.

Biographical Memoir of Roland Thaxter. (1858–1932.) Natl. Acad. Sci. **17:** 55–68. 1936.

Notes on a report on pruning conifers as carried out in Eli Whitney Forest. Quart. Jour. Forestry **30(1):** 45–51. 1936.

Connecticut Agricultural Experiment Station Bulletins of Immediate Information

Blight and rot of potatoes: **2.** 1906.

Root rot of tobacco: **4.** 1906 (with E. H. Jenkins).

Winter pruning of fruit trees: **19.** 1923 (with W. E. Britton).

Dormant sprays on orchard trees: **20.** 1923 (with W. E. Britton).

The pink spray for apple orchards: **22.** 1923 (with W. E. Britton).

Diseases carried by seed potatoes: **23.** 1923.

The calyx spray for apples, pears and quinces: **24.** 1923 (with W. E. Britton).

Winter conditions of apple and peach buds: **28.** 1924 (with E. M. Stoddard).

Dormant sprays for orchard pests: **29.** 1924 (with W. E. Britton and Philip Garman).

Information about insecticides and fungicides: **30.** 1924 (with E. M. Stoddard, W. E. Britton and Philip Garman).

Why and how to spray: **31.** 1924 (with E. M. Stoddard, W. E. Britton and Philip Garman).

Varietal susceptibility of apples to diseases and injuries: **32.** 1924.

The pre-pink and pink sprays for apples: **33.** 1924 (with M. P. Zappe).

Grape spraying: **38.** 1924 (with E. M. Stoddard).

Spraying potatoes: **42.** 1924.
Sun scorch, anthracnose, etc., of shade trees: **45.** 1924.
Prematuring of vegetables, rots of lettuce and similar troubles: **46.** 1924 (with Florence A. McCormick).
Prematuring and other potato troubles: **47.** 1924.
Stewart's bacterial wilt on sweet corn. Circ. **96.** 1934 (with W. Ralph Singleton).

Proceedings of the Connecticut Pomological Society

Some diseases of the apple. Report for **1902:** 33–45. 1903.
Report on fungous diseases for **1903:** 19–26. 1904.
Report on fungous diseases for **1904:** 34–37. 1905.
Report on fungous diseases for **1905:** 41–44. 1906.
Report on fungous diseases for **1906:** 23–27. 1907.
Report of committee on fungous diseases for **1907:** 30–35. 1908.
Report on fungous diseases for **1908,** with special notes on melon culture and diseases, and the peach yellows: 39–51. 1909.
Report on fungous diseases for **1909:** 150–153. 1910.
Experiments in controlling diseases of apples and peaches. Report for **1910:** 50–58. 1911.
Results of spraying experiments in 1911, on apples and peaches. Report for **1911:** 188–198. 1912.
Report of committee on fungous diseases for **1912:** 25–29. 1913.
Fungous diseases in Connecticut in **1913:** 71–81. 1914.
Fungous diseases in Connecticut in **1914:** 28–40. 1915.
Fungous diseases in Connecticut the past year. Report for **1915:** 40–43. 1916.
Report on fungous diseases in Connecticut the past year. Report for **1916:** 24–28. 1917.
Report of fruit diseases for **1918:** 101–107. 1919.
Report on diseases of fruits for **1919:** 18–24. 1920.
Fruit diseases in **1920:** 28–36. 1921.
Connecticut fruit troubles in **1922:** 10–13. 1922.
Injuries and diseases of Connecticut fruits in **1923:** 37–41. 1924.
Recent studies on certain apple diseases and their control. Report for **1924:** 65–73. 1925.
Injuries and diseases of the peach in Connecticut. Report for **1925:** 29–37. 1926.
Diseases and injuries of Connecticut fruits in **1926:** 31–34. 1927.
Diseases and injuries of Connecticut fruits in **1927:** 19–24. 1928.

Connecticut Vegetable Growers' Association Reports

Report of diseases of market garden crops in **1916:** 7–11. 1917.
Report on fungous diseases for **1917:** 14–15. 1918.

Diseases of vegetables in **1919**: 54–60. 1920.

Diseases of vegetables in **1920**: 22–24. 1921.

New facts concerning diseases of vegetables and their control. Report for **1921**: 7–20. 1922.

Connecticut vegetable diseases in **1922**: 69–74. 1923.

Diseases of Connecticut vegetables in **1923**: 45–48. 1923.

Dusting *versus* spraying of celery. Report for **1923**: 54–65. 1924.

Diseases of vegetables, spinach blight, and celery spraying. Report for **1925**: 19–29. 1925.

Diseases of Connecticut vegetables in **1926**: 26–29. 1927.

Diseases and injuries of Connecticut vegetables in **1927**: 70–73. 1928.

Dusting *versus* spraying. Report for **1928**: 15. 1928.

Report of plant disease committee. Report for **1928**: 20–22. 1928.

Some of the worst vegetable diseases of Connecticut. Report for **1929**: 51–55. 1929.

Practical treatment for the common fungous diseases of our vegetable crops. Report for **1930**: 34–37. 1931.

Plant diseases in Connecticut. Report for **1933**: 46–50. 1934.

Stewart's bacterial wilt of sweet corn. Report for **1933**: 69–72. 1934.

Vegetable diseases in **1936**: 51–54. 1936.

Edwin O. Jordan

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WILLIAM BURROWS

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EDWIN OAKES JORDAN

1866–1936

By William Burrows

Edwin Oakes Jordan was born July 28th, 1866, and died September 2nd, 1936. During his lifetime his chosen fields, bacteriology and public health, developed from their beginnings to their present outstanding position among the scientific activities of today. Although the initial contributions of Pasteur and Koch preceded Jordan's college days by a few years, the new knowledge was slow in reaching America in the form of actual practice and was beginning to come with increasing force when he started his college work. The effect of the heady stimulation of the new science of bacteriology on the young man, and the application to it of his own unusual abilities throughout the remainder of his life, go to make the development of the man and the scientist a brilliant facet of the history of the growth of bacteriology in this country.

Jordan was born at Thomaston, Maine. His grandfather, Oliver Jordan, was a master-mariner and shipowner who, after he retired from active life, lived in a dignified and beautiful home on Main Street, the street of fine houses in Thomaston. His father, Joshua Lane Jordan, was one of nine children and, like many others in his day, went to sea at an early age. By the time he was twenty-one he, too, was a captain and commanded first one and then another of the merchantmen then building at Bath and Thomaston.

Captain Joshua Jordan married his third wife, Eliza D. Bugbee, in 1865. When their first child, Edwin Oakes, was six months old, the father took command of the vessel, *Pride of the Port,* sailing from Thomaston. The family, augmented at Bombay by another child, did not return to this country for three years. Their home was, for the most part, the captain's cabin on board ship, with occasional brief stays ashore at such ports as Liverpool and Bombay. When Edwin Oakes was three and one-half years old, his father retired from seafaring, settled down in Thomaston in a house close to the grandfather's home, and engaged in the local banking business.

Mrs. Jordan was considerably younger than her husband. She was of Puritan stock which settled in this country early in the seventeenth century. After several years in the Normal Training School at Framingham, Massachusetts, she had taught in a country school. Although exceedingly shy and retiring, she was a woman of strong character, a devout Christian with a high sense of duty, with great potentialities for self cultivation and a saving sense of humor. She took a very active part in the education of her four children, interesting them both in good literature and in natural science. Jordan's self-effacing character, combined with an inner force which caused him to push forward any project that he felt to be good, without regard to the cost to his natural timidity, was undoubtedly derived directly from his mother. It was through her that he began a collection of minerals, an avocation that afforded great delight in his boyhood and a keen secondary interest throughout his life. During a number of his adolescent years he intended to become a mineralogist or geologist.

Thomaston was a small town in the seventies but a very busy and prosperous one with its shipbuilding, lime-quarrying and lime-burning industries. The life of the boy there was full of pleasant incidents, such as picnics upon land, mineralogical expeditions behind the pony, Nebuchadnezzar, and rowing and sailing a boat on the tidal river. Thomaston did not, however, present very good educational opportunities and on this account the family moved to Auburndale, Massachusetts, in 1881 where Jordan attended and graduated from the Newton High School. In 1884 he entered the Massachusetts Institute of Technology.

Already interested in natural science through his mother and having a nodding acquaintance with the methods of science through his collection of minerals, Jordan came in contact with William Thompson Sedgwick. Sedgwick exerted a strong influence on the young man, an influence whose impress remained with him throughout his life. This was evident not only from his continuous and lifelong devotion and loyalty to Sedgwick, but even more in his habits of thought and his marked tendency to think not only of the immediate consequences of experimental observations but, further, their relation to broad biological prin-

ciples. Undoubtedly this characteristic was innate in Jordan's own mind and very possibly his semi-philosophic attitude formed the nucleus of the close bond between him and Sedgwick.

Sedgwick was himself a young man, having been appointed Assistant Professor of Biology only the year before Jordan entered college. He became intensely interested in the newly born science of bacteriology and quickly grasped the significance of the new facts. The golden age of bacteriology was at its heyday in the eighties. New discoveries tumbled one after another with bewildering rapidity—the discoveries of the tubercle bacillus, the typhoid bacillus, the diphtheria bacillus, the meningococcus—and the giants of the day, Pasteur, Koch, Loeffler, Weichselbaum, Kitasato, Gaffky, Eberth, Pfeiffer, Roux and the rest, were not names only but active workers. Small wonder that those who took their first courses in biology with Sedgwick felt that a new world was opening up before their eyes. Jordan's active mind responded vigorously to this stimulation. It is much to his credit that he was not carried away in the whirl of medical discoveries but kept a solid footing on purely biological foundations. His interests followed those of Sedgwick into the sanitary and hygienic significance of the new knowledge, a field which remained of first importance to him all his life.

The year of Jordan's graduation, 1888, Sedgwick was appointed Consulting Biologist to the newly organized Massachusetts State Board of Health. Through Sedgwick, he was appointed Chief Assistant Biologist at the Lawrence Experiment Station and was intimately associated with the early experimental work carried out there on sewage and on the filtration of water. Before beginning the work at Lawrence, however, he spent two months with T. Mitchell Prudden at the College of Physicians and Surgeons in New York. Prudden, stimulated by the current discoveries in bacteriology, had gone back to Germany and spent a month in 1885 studying under Koch and had returned to America with the latest information. From him Jordan obtained first hand information on the differentiation of the typhoid and colon bacilli, the use of agar in semi-solid media, the Gram and Ziehl-Nielsen stains and other technics in use in Koch's laboratories. Something in Prudden's reticent but rich personality

appealed strongly to Jordan and the former's influence did much to augment the interest aroused by Sedgwick.

For the next two years, until 1890, Jordan carried on an intensive experimental study of the bacteria present in water and sewage. His efforts were devoted for the most part to the application of Koch's semi-solid gelatin medium to the study of flora characteristic of water and sewage. In later years he often spoke of the tedious methods used in the preparation of plates —the mixture of gelatin and water sample was poured on chilled rectangular plates of window glass and, after hardening, these were placed in tiers in shallow covered glass jars. Liquefying colonies, melted plates and other hazards made counting difficult and on a number of occasions in later years he remarked with solemn visage but a twinkle in his eye that "the method did not appeal to the engineers as highly accurate."

Two important points emerged from his work. One of these, the constant presence of the colon bacillus and closely related forms in sewage—and the absence of these organisms from water known not to have been exposed to sewage contamination —assumes great significance in the light of subsequent work. That he had hopes of finding a reliable biological indicator of pollution is evident from his remarks at the time, ". . . a study of the sewage bacteria as such may throw light on the vexed question of the possible pollution of water supplies; for, if certain species are found to be characteristic of sewage, and are never found in uncontaminated sources, then the presence of these typical 'sewage bacteria' in any given water supply will indicate undoubted pollution. We may perhaps look forward to the time when the bacteriologist will be able to say, of a given water: Such and such species of bacteria are present, therefore, at some time sewage must have entered this water; or, on the other hand: Only those species are present which are always found in pure uncontaminated water."

The other point has been generally overlooked by subsequent investigators. Jordan, with Ellen H. Richards, studied the nitrifying bacteria of soil and water. Following the work of the Franklands and others, they reached the conclusion that the microorganisms would not grow on gelatin plates and succeeded

in isolating cultures of nitrifying bacteria in inorganic solutions. Shortly before the experiments were complete, Winogradsky's first report appeared and Jordan's independent work had only the status of confirmation although published in the same year. He and Mrs. Richards were undoubtedly the first in America to study nitrification.

Meanwhile he had spent a summer or two at Woods Hole and there met Charles Otis Whitman and some of the other zoologists with whom he was to be associated later. As a result of Whitman's influence, Jordan became interested in zoology and experimental embryology and through him obtained a two year fellowship at the newly founded Clark University in Worcester, Massachusetts. He went to Clark in 1890 to finish his formal training with Whitman and received his Ph.D. in 1892. The new environment was highly congenial in many ways. Clark University had been founded by G. Stanley Hall for the purpose of establishing a strictly graduate and research institution. Whitman's own mind had much in common with this ideal. Speaking of the graduate student, he had said, "He is recognized, not as an irresponsible school-boy, to be marked for absences, ranked for recitations, and rewarded, after a prescribed number of years of study and decent behavior, with a 'graduating degree'; but as a man who knows, or ought to know, his purpose, and who, if he ever expects to attain the distinction of a degree, must demonstrate his eligibility thereto by making some worthy contribution to the advancement of knowledge in his chosen field." This ideal, together with Whitman's strong conviction that a young man had best be given a problem and left largely to his own devices in working it out, were in complete accord with Jordan's own attitude. Despite the discrepancy in their ages—Jordan was twenty-four and Whitman fifty-eight—Jordan developed a great admiration for the older man and was undoubtedly greatly influenced by him. Both were interested in fundamental rather than superficial biological phenomena and shared the conviction that devotion to research was a prime means and chief end of higher education. Jordan developed a warm friendship with the other younger men, Shosaburo Watasé, Frank R. Lillie and William Morton Wheeler, and his associa-

tion with them went far in adding to his enthusiasm for his scientific work and in keeping his mind out of what might readily have become a narrow rut of sanitary biology. He was engaged at the time to Elsie Fay Pratt, whom he married shortly afterwards.

The experimental work he did at Clark University was reported in two papers. One of these was a study of the spermatophores of Diemyctylus and the second, entitled "The Habits and Development of the Newt," was his doctor's thesis. In the latter piece of work he supplemented the usual histological methods of study by continuous observations on the living embryo during gastrulation, and was able to see the movement of surface cells over the lips of the blastopore. This and other observations led him to support the theory of invagination rather than that of delamination in the formation of the mesoderm—a conclusion which has been strongly supported by later work.

When Whitman was offered the chair in zoology at the newly organized University of Chicago, Jordan readily accepted the invitation to accompany him and the other men in the department to Chicago as an instructor in zoology. The University opened in the fall of 1892 and Jordan began the active part he played in the development of the institution with which he was to remain the rest of his life. He was twenty-six years old when he went to Chicago.

Jordan's interest in the rapidly developing science of bacteriology and the application of the new knowledge and its methods to preventive medicine soon became evident in concrete form. In the spring of 1893, the first year he was at the University, he gave a course entitled "Sanitary Biology" which the announcement for that year describes as "The sanitary problem. The methods, objects, and results of the examination of drinking water; the examination of air, soil, milk, ice, etc. Sewage disposal and water supply. The filtration and precipitation of sewage. The nitrification of organic matter. Lectures and seminar." Wheeler gave the courses in embryology, Watasé those in cytology and Lillie finished his graduate work.

The following year, 1893-'94, Jordan expanded the course in sanitary biology to two courses, one in general bacteriology and

the other in advanced bacteriology. He gave, in addition, a course in general biology. He had already started experimental work in bacteriology, a study of the typhoid bacillus and the methods of differentiating and identifying it. He had not altogether abandoned experimental zoology at the time, for in 1894 he published with Eycleschymer a study of the cleavage of amphibian ova. After this, however, he devoted himself to bacteriology entirely insofar as his own experimental work was concerned and published no more work in pure zoology.

The next year he was made an assistant professor. He gave the same courses and continued his experimental work with the typhoid bacillus, a study of the conditions affecting its behavior in water. He went to Europe for a short time in the spring of 1895 and spent the six weeks he had available there at the Pasteur Institute in Paris. Although there was no time for extended experimental work, the stimulation and the contacts with men such as Roux and Duclaux were valuable and he took full advantage of the opportunity to obtain first hand information with regard to European practice. He returned to Chicago for the year 1895-'96, gave again his courses in general biology and elementary zoology but expanded the work in general bacteriology to two courses, one introductory and dealing with the biological and sanitary aspects of bacteriology and the other covering the pathogenic bacteria, disinfection, *et cetera*.

By 1897-'98 bacteriology at the University of Chicago began a rapid expansion under Jordan's direction. Charles Manning Child joined the faculty and took over the course in elementary zoology, leaving Jordan more time to devote to bacteriology. Albert Lincoln Smith, who had received a Ph. D. from the University of Berlin seven years before, registered as a special student in the University and gave a course in water and water supplies. Howell Emlyn Davies, a graduate student who held a fellowship in bacteriology, gave a course in elementary bacteriological technic. Jordan continued with general and pathogenic bacteriology and, in addition, introduced a seminar in immunity. The lack of texts of bacteriology in English for the benefit of those who did not read German with facility had been evident for some time, and Jordan attempted to fill in the

gap with a translation of Ferdinand Hueppe's well-known "Naturwissenschaftliche Einführung in die Bakteriologie" into English under the title of "The Principles of Bacteriology." The translation was published in 1899 and although regarded as a remarkably good translation of difficult, idiomatic German, did not enjoy a great popularity. Its lack of popularity undoubtedly had roots in Hueppe's highly individualistic concept of disease. He differed from Koch in that he felt that disease was a process resulting from varying causes and that on dynamic principles the most important cause was to be sought in the structural idiosyncrasies of the patient rather than in the invading bacteria which he regarded, at the most, as "liberating causes."

Jordan had, by this time, become thoroughly convinced of the importance of bacteriology as a separate science rather than a branch of some other biological science, and felt strongly that bacteriologists should have a society of their own. He was not alone in this, of course, but he and H. W. Conn of Wesleyan University were the only ones who campaigned actively for the organization of such a society. Their efforts were so successful that the first meeting of the Society of American Bacteriologists was held in New Haven in 1899 with a program arranged by A. C. Abbott. Jordan was very active in the Society for a number of years, holding the office of president in 1905, but in later years his active participation declined. His part in the organization of the Society was, perhaps, as typical of the man as any other single project. He felt that such a Society was actively needed and he spared no effort and inconvenience to get it under way. He actively supported it in its early struggling days, but when it became clear that the Society had developed into a healthy, strong organization, he characteristically left it to be conducted by the younger men, but always retained a warm personal interest in it.

Meanwhile the work at Chicago was developing rapidly. Jordan was made associate professor in 1899, the last year bacteriology stayed in the Department of Zoology. Davenport had become a member of the staff and took over the course in general biology so Jordan's teaching time could be devoted entirely

to bacteriology. He made use of this additional time by offering a course in public hygiene and found it necessary to have Davies help him with the general course. Smith again gave the course on water and water supplies. Jordan's own experimental work continued with an extensive study of *Bacillus pyocyaneus* and its pigments and another study of the production of fluorescent pigments by bacteria. His broad, general biological view of bacteriology is evident not only in his papers on bacteria, but even more in the variety of aspects of bacteriology and preventive medicine which interested him. In 1898 he read a paper before the Chicago Medical Examiners Association on the supposed inheritance of bacterial diseases and in 1899 published a study of the death rate from diphtheria in the large cities of the United States. It was clear to him from the beginning that preventive medicine and hygiene depended not only on purely bacteriological and immunological studies but also on the general biological aspects of host and parasite populations, the mechanisms involved in the transmission of infective agents and many other factors.

In 1900 bacteriological work was removed from the Department of Zoology and incorporated in a new Department of Pathology and Bacteriology, of which Ludvig Hektoen was in charge. The change was with respect to administration, for the laboratories continued to be housed in the Hull Biological Laboratories. Bacteriological work had expanded to such an extent that the entire fourth floor of the Hull Laboratories was occupied. The change, of course, included a change in the staff with whom Jordan was associated. Smith and Davies no longer worked in the bacteriological laboratories and Jordan's associates in the new department included H. Gideon Wells and William Buchanan Wherry in addition to Hektoen. Wherry was an assistant in bacteriology. Very much the same work in bacteriology was offered—courses in general and pathogenic bacteriology, elementary technic, public hygiene and research. Ernest E. Irons, Howard Taylor Ricketts and Mary Hefferan came to the department the following year, and Wilfred Hamilton Manwaring in 1903.

During these years a situation developed in Chicago which was, in a sense, made to order for Jordan with his training and experience. For many years the city of Chicago had been discharging its sewage into the Chicago River and, as the population increased and the volume of sewage grew, the water intakes were moved farther and farther out into the lake to escape sewage contamination. By the late eighties, the situation had become critical. Typhoid fever was common in the city, a serious epidemic occurred in 1890-'91, and minor epidemics were frequent. In a report of an investigation of the quality of public school water supplies conducted by Jordan in collaboration with F. R. Zeit and J. H. Long of Northwestern University, he wrote ". . . Through most of the time during the period covered by our examinations the water has not been in a safe sanitary condition. . . . It should be added that its condition now is probably not worse than has been the case many times in the last fifteen years. . . . The real source of the difficulty is in the quality of the general public water supply. . . ." The contaminated water supply assumed somewhat greater importance to the city at this time because of the approaching World's Fair It was so bad that the *Lancet* went to the length of appointing a commission to study the sanitary aspects of Chicago city water with the object of providing unbiased information for Englishmen who might come to the Exposition. Needless to say, the report of the commission was not flattering. Meanwhile the city had taken action and had organized the Sanitary District of Chicago under a general law enacted by the State Legislature in 1889. The remedy applied through the agency of the District was the cutting of the Drainage Canal from Bridgeport to Lockport and thereby reversing the flow of the Chicago river. Sewage was, therefore, no longer discharged into the Lake but drained into the Mississippi River by way of the Illinois and Desplaines Rivers. The remedy was efficacious as far as Chicago's water supply was concerned but resulted in a suit between St. Louis and Chicago in which the former charged that the sewage which eventually found its way into the Mississippi River resulted in contamination of the St. Louis water supply.

Jordan was intimately associated with this sewage disposal scheme from its beginning and carried on extensive bacteriological examinations of the water of the Illinois River at various points both before and after the Canal was opened. His investigations were the first controlled and extensive examinations of the question of self-purification of streams and provided a solid foundation of fact upon which all later work has been based. Briefly, he found that the enormous numbers of colon bacilli present in Chicago sewage disappeared completely in less than one hundred and fifty miles of flow and that the bacterial flora of the Illinois River at Grafton, where it empties into the Mississippi, was not altered by the opening of the Drainage Canal. His testimony on this point before the Supreme Court of the United States was the decisive factor in its decision in favor of the city of Chicago. Jordan's keen mind was nowhere displayed to better advantage than on the witness stand before the Court. Aside from the legal implications of his work, he had established the fact of self-purification of streams, a tremendously important addition to knowledge of sanitation and one upon which innumerable sewage disposal systems have since been based. It was not alone the scientific interests of the matter that motivated his work. He remarked on another occasion that "It is one of the vital offices of a university to contribute to the well-being of the community in which it is placed." His contributions to the sewage disposal of the city were, in effect, a partial discharge of the obligation he felt, and he continued to discharge this obligation further in following years through his association with the public health activities of the city. The present high standard of public health in Chicago is due, in large part, to his influence and advice.

Since the translation of Hueppe's text of bacteriology had failed to satisfy the need for an adequate text in English, Jordan filled the gap by writing one of his own. The idea had undoubtedly been germinating for some time, and in 1902 he entered into an agreement with the W. B. Saunders Company for the publication of a text which he would write. His book, "General Bacteriology," was a carefully considered and carefully written volume, one which he took six years to write—the

first edition appeared in 1908. His facility for writing beautiful English stood him in good stead and the immediate and continued success of his text is no doubt attributable to its well written, orderly and accurate presentation, as well as to the need it filled. For many years Jordan's "General Bacteriology" was, by all odds, the most widely used text in this country and had gone through eleven editions at the time of his death. Through the agency of this volume, Jordan may be said to have exerted a strong and wide influence on the development of American bacteriology and it was one of his important contributions to the field.

The John McCormick Institute for Infectious Diseases had been founded in 1902 by Mr. and Mrs. Harold F. McCormick. Ludvig Hektoen was director of the Institute and in 1905 Jordan was placed in charge of the Serum Division. At the time antitoxic sera were prohibitively high in price and altogether beyond the reach of the general public. The situation appeared unreasonable and Jordan was moved to undertake the production of diphtheria antitoxin under the auspices of the Institute so that such sera might be made available generally. His mind, of course, went beyond the immediate issue, the therapeutic use of antitoxin in the individual case, to the more general public health aspects of diphtheria control. He personally purchased a farm outside the city in Barrington, Illinois, and there the Institute kept horses for the production of antitoxin. Paul Gustav Heinemann, who had come to the department as a fellow in 1904, gradually took over the actual supervision of the work at Barrington, thereby relieving Jordan of much of the detail. Only native serum was produced at the beginning, but in 1907 the Gibson method of refining and concentrating antitoxin was adopted and a few years later the Banzhaf method, one utilizing plasma instead of serum, took the place of the Gibson method. A modification of the latter method was worked out and a more efficient method of bleeding the horses developed. Better serum than that available commercially was produced under Jordan's direction, and the improved methods were published to make them available to others. At first antitoxin was distributed through the Illinois State Board of Health but later, when more

became available, the Serum Division changed its policy so that physicians and druggists could obtain antiserum directly from the Institute or from the City of Chicago Department of Health. The costs were borne by the Institute and the antitoxin was purchased from the Institute, Jordan contributing his time and the use of the farm. It is difficult to estimate the benefit conferred by the serum work as carried out by the Institute. Not only were indigent sufferers from the dread disease relieved but, further, the products of the large manufacturing concerns were materially improved and reduced in cost. The Serum Division was discontinued in 1917 after its purposes had been accomplished.

Jordan was made full professor in 1906. For a period of several years, roughly from 1905 to 1914, his interests went further into the field of public health. His work with water-borne disease took the form of specific investigations of outbreaks of typhoid fever in Milwaukee, Detroit, Des Moines, St. Charles, Winnipeg, Quincy, Rockford and other municipalities. His papers reporting these investigations are models of what an epidemiological investigation should be, and he was invariably able to put his finger on the weak link in the sanitary chain. The detailed bacteriological studies in connection with these epidemics completed the picture of the mechanisms involved in the transmission of the disease. The study of the Rockford epidemic was one of the most interesting of these, for here Jordan found definite evidence that the contamination of the water supply with sewage sometimes resulted in a preliminary gastro-enteritis of relatively short incubation period which preceded the actual infections. As a result of these and other pieces of work, he came to be regarded as one of the foremost authorities in the country on water-borne disease and at the request of the United States Public Health Service, he set up bacteriological standards for drinking water supplied to the public by common carriers in interstate commerce.

He became more and more interested in milk and milk-borne disease, incidentally tracing three outbreaks of typhoid fever to contaminated milk supplies. He was very active in the campaign for pasteurized milk in Chicago. One of his earliest pieces of

work in this connection (1904) was a pointed analysis of the Chicago milk market, carried out while he was a member of the Health and Sanitation Committee of the Civic Federation of Chicago. As a member of the Committee on Regulations for the Pasteurization of Milk, he was, in large measure, responsible for the organization and drafting of a code of uniform and effective practice. Further, he took an active part in creating an informed public opinion on the desirability of pasteurization of milk supplies through articles such as "The Campaign for Pure Milk," published in Christendom, "The Household Milk Supply," a publication of the Domestic Science Department of the University, "The Case for Pasteurization," in the Journal of the American Medical Association, and similar informative writings which reached the medical profession and various portions of the general public. His own enthusiasm was, of course, for the adoption of effective public health measures based upon a solid foundation of scientific fact, and he realized that the hope of achieving such goals lay in the education of the public at large.

In spite of the fact that many of his activities were directed toward specific goals, Jordan never became narrow in his interests. In the midst of studies of typhoid fever epidemics, milk-borne disease and other investigations, he found time for articles such as "The Sphere of Bacteriology," "The School and the Germ Carrier," "Profitable and Fruitless Lines of Endeavor in Public Health Work," "School Diseases," "Disease Carriers Among School Children," and others. In 1912 he began the series of annual studies on typhoid fever in the large cities of the United States which appeared anonymously in the Journal of the American Medical Association.

Meanwhile his experimental work continued along various lines. His interest in insect-borne disease is apparent from two pieces of work on anopheline mosquitoes, one a note on the occurrence and habitat of *A. punctipennis* and *A. maculipennis,* and the other a study on the binomics of *Anopheles.* From the beginning of his work and throughout his life, Jordan was greatly interested in what is often called "pure" bacteriology. He carried on active experimental work of this kind in addition to his public health activities. Work on bacterial

enzymes, on the effect of bile on the colon bacillus, and similar studies may be considered to be in this category. Perhaps one of the most interesting evidences of the breadth of his scientific thinking during this period is a paper on bacterial variation read before the National Academy of Sciences. The strict monomorphism of Koch and his school had, for a time, subdued pleomorphism with the exception of that exhibited by the so-called involution forms. Bacterial variation did not come into its own until the early twenties with the work of de Kruif and Arkwright, and yet in 1914 Jordan perceived the importance of the problem and discussed it at length. His peculiarly analytical mind, which solved complex epidemiological problems in what seemed to many an almost uncanny fashion, is evident through all his work. His early study of a thermostable hemolytic substance present in sterile nutrient broth, an attempt to assay the significance of such variables in studies on bacterial hemolysins, was thoroughly characteristic of the man. Those who worked with him knew of his constant, almost fanatical, demand for control experiments. During this period Jordan became much interested in milk-sickness and carried on extensive experimental work in collaboration with Norman McLeod Harris in an attempt to find a bacterial etiology. These attempts were unsuccessful and it was later shown that the toxic qualities of milk from cows with trembles were a result of poisoning of the animals by white snakeroot.

In 1909-'10 he spent a sabbatical year at Freiburg. During the year he did little or no experimental work but spent his time studying the sanitary organization of the larger German cities, such as Frankfort, Berlin and others. Germany was, of course, much further advanced than the United States in this respect, and Jordan was particularly interested in the methods used in training sanitarians and the organization of instruction in hygiene. The information he gained was put to good use when he returned to America.

For some time Jordan had felt that the scope of bacteriology was so broad that its possibilities, particularly with regard to hygiene and preventive medicine, could not be realized in a Department of Pathology and Bacteriology. The rapidly grow-

ing importance of preventive medicine convinced him that instruction in this field should be made available in medical schools, and the medical school at the University in particular, and further that an opportunity should be provided for the training of health officers and experts in the field. His efforts and enthusiasm bore fruit in the creation, in 1912, of the new Department of Hygiene and Bacteriology with Jordan as head of the Department. The change was, again, one of administration, for the laboratories continued to occupy the fourth floor of the Hull Biological Laboratories. The first year the staff included, in addition to Jordan, Harris who had been in the old department since 1902, Heinemann, and Wherry, who had left but returned as a visiting professor. Wherry gave a course in parasitology, the first in the University, and Jordan expanded his own teaching to include a course on vital statistics and epidemiology. The introduction of parasitology into the Department was an innovation in the field of bacteriology. The protozoan and helminth infections and the role of insects in the transmission of disease had, of course, been known for many years, but Jordan was one of the few who perceived the essential similarity and common ground between the two fields. In later years it has been more generally realized that parasitic and bacterial infections have much in common, not only in modes of transmission, but also in the defensive mechanisms of the host against the infective organism which, in many cases, appear to be identical. Jordan hoped, from these beginnings, to develop a school of hygiene and public health that would function side by side with investigative work of a fundamental nature. This ambition was never quite realized, owing to a variety of circumstances, but the plan served as a stimulus for the development of such work in American universities.

By 1915 the Howard Taylor Ricketts Laboratory had been built by the University to serve as temporary quarters for the Departments of Pathology and of Hygiene and Bacteriology, and after twenty-three years bacteriological work at the University was physically separated from the Department of Zoology, where it had originated. The new laboratory was named in honor of Ricketts, a member of the faculty of the Department

of Pathology and Bacteriology who had died in 1910 of typhus fever during an investigation of that disease in Mexico.

During these years Jordan's interests had become even broader and included the field of food poisoning. The transition from the water- and milk-borne enteric diseases to gastro-enteritis resulting from the ingestion of foods was not a difficult one. The expansion of his interests in this direction was undoubtedly facilitated by his contacts with the large meat packing concerns in Chicago. He had been asked by one of these companies to investigate a stubborn outbreak of typhoid fever in a subsidiary plant in South America. He solved the problem in his usual competent fashion and at the same time developed a marked interest in the problems of food preservation and food poisoning which confronted the packing industry. This interest soon took concrete form in the shape of several publications in the general field of the bacteriology of foods, food-borne infections, and food-poisoning. He began at this time his association with the packing industry as an advisor and consultant, an association which lasted until his death. The step took courage on his part for, at the time, it was generally felt that industrial connections were not altogether desirable for one engaged in academic work. The opportunity to contribute further, although indirectly, to public health was not to be denied and the sanitary improvements resulting from close cooperation between industry and the research laboratory have amply sustained his feeling.

About this time Jordan started an extended investigation into the differentiation and biological characteristics of bacteria of the typhoid-paratyphoid group. The morphological, physiological and immunological similarities of these organisms made differentiation of the species from one another a difficult matter and a possible solution lay in a detailed and careful study of the entire group. The results of this work were embodied in a series of papers extending over a period of years, the last paper appearing in 1925. As a result of this investigation and a number of others, he became the foremost authority on these organisms in America.

When the United States entered the World War in 1917 two sanitary needs were apparent at once; a supply of trained tech-

nicians for laboratory diagnosis, and organization and supervision of the laboratories at the training camps in this country. The problem which arose was the control, not of enteric infections as in previous wars, but of respiratory infections, such as pneumonia and epidemic influenza and of meningococcus meningitis. The last was of greatest importance in the early days, the influenza control came later. Jordan, in common with the other bacteriologists of the country, rose to the occasion and bent every effort toward the control of these diseases in the army camps. His work took two forms; one, the most obvious, was the training of technicians for work in the army laboratories. The University laboratories were used to a great extent for this purpose, the courses of training being arranged and taught under Jordan's direction. The other work he undertook was in the capacity of director of the Red Cross car "Lister." Four of these cars, equipped for field laboratory work, were built by the Pullman Company and operated by the Sanitary Service of the American Red Cross. Jordan made a series of trips in the "Lister" to army camps in various parts of the country. When the laboratory of a given camp required organization or instruction in diagnostic methods or when an epidemic appeared to be getting out of hand, the car was called and whatever measures necessary were taken. The "Lister" operated only a few months before being turned over to the army along with the "Reed" and "Metchnikoff." One car, the "Pasteur," was retained by the Red Cross and Jordan accepted the directorship of this car after the others had been transferred. The essential weaknesses in the organization for control of disease in the army camps were apparent at once to Jordan, and he made many pointed and valuable suggestions which were perhaps of considerably more importance than the actual work of laboratory organization and instruction in diagnostic procedures.

The influenza pandemic of 1918-'19 was of great concern, not only to the army but to the civilian population as well. The problems confronting the bacteriologists were difficult ones, particularly since Pfeiffer's bacillus, thought to have been established as the causative agent of influenza many years before, was found to bear only an uncertain relation to the disease. As

a member of the Respiratory Commission, Jordan undertook a systematic investigation of the bacteriology of the disease, an investigation in which not only he but other members of his department took an active part and which extended over a period of several years. Other members of the Commission made similar investigations and by frequent consultation and pooling of information it was hoped that some light might be thrown on the etiology of the disease. The venture was not successful, however, and in 1927 Jordan wrote an extended review entitled "Epidemic Influenza" which was published in book form by the American Medical Association and which did much to clarify the chaotic mass of information which had accumulated about the disease in the course of years. His serious consideration of certain experimental evidence suggesting a virus etiology is of particular interest in view of the recent work in which a filterable virus has been shown to be the cause of at least some kinds of influenza.

After the brief disorganization of the war period, the department settled down to continued development under Jordan's guidance. An additional laboratory, Ricketts Laboratory South, had been built and a few years later, when the Department of Pathology moved to new quarters in Billings Hospital, the Department of Hygiene and Bacteriology occupied both buildings. The work in parasitology, begun with Wherry in 1913, took more definite form with the addition of William Hay Taliaferro to the staff in 1924. Taliaferro initiated and maintained active research in the field which in its fundamentals drew closer to bacteriology.

Not long afterwards, research on virus diseases was initiated in the department—at first confined to poliomyelitis but later, with the addition of N. Paul Hudson to the staff, extended to include a variety of studies on other diseases of virus etiology.

Jordan's own work on the paratyphoid-enteritidis group and his epidemiological work went on unchanged. He undertook an annual report on diphtheria mortality in the large cities in the United States, a companion to the annual typhoid report, which was likewise published anonymously in the Journal of the American Medical Association. He prepared standard methods of

bacteriological analysis of milk for the American Public Health Association and published papers on the bacterial content of stored normal and typhoid feces and the interconvertibility of "rough" and "smooth" bacterial types. His experimental work turned to studies on food poisoning and food-borne infection and the relation of the paratyphoid bacilli to these problems. He had, by this time, become accepted as the first American authority on food poisoning and shared international honors only with Savage of England. The food poisoning investigations took a new and promising turn with Dack's discovery of a filterable substance produced by staphylococci which, on feeding to human beings, produced the typical clinical picture of food poisoning. The importance of this observation was obvious to Jordan and the phenomenon was subjected to an intensive investigation. Jordan's observation that certain monkeys were susceptible to the action of the toxic substance was soon confirmed by the other workers in the laboratory and put the study of the toxic substance on a solid experimental foundation. He later found that a variety of bacteria, including the presumably innocuous colon bacillus, could produce similar toxic material under suitable conditions of cultivation. This finding was of some interest in that it was in complete accord with epidemiological studies. Savage had previously postulated the existence of such toxic substances on theoretical grounds, an hypothesis with which Jordan did not agree until their existence could be shown experimentally. The earlier portions of this work were summarized, in a general review of food poisoning in book form entitled "Food Poisoning and Food-Borne Infection," published in 1931. The volume was, in effect, a second edition of an earlier book, "Food Poisoning," published in 1917. Characteristically, in neither of these did Jordan regard food poisoning as a purely bacteriological problem but presented, in addition, a complete and concise summary of food poisoning resulting from contamination of food with toxic chemical substances.

In the last few years of his life, Jordan took his vacations in the winter and spent them in Puerto Rico, Panama and Jamaica. At no time did he stop working, for in Puerto Rico he was visiting professor in the School of Tropical Medicine, in the

Canal Zone he worked in the Gorgas Memorial Laboratories—it was here that he discovered the susceptibility of monkeys to the staphylococcus toxic substance. In Jamaica he became interested in an affection peculiar to that island and certain other parts of the world which was called "vomiting sickness," and by a judicious combination of epidemiological and experimental work, disclosed significant facts relating to its etiology. While at these places he gathered information regarding the general sanitary situation which he presented to the Rockefeller Foundation in the form of informal reports—he had been a member of the International Health Division for some years and then a member of the Board of Scientific Directors of the Foundation.

Jordan was appointed Andrew McLeish Distinguished Service Professor of Bacteriology in 1931 and was retired in 1933 at the age of 67. He continued to work as actively as ever after retirement but was not well during the last year of his life, his illness the result of a coronary occlusion. During the latter part of the summer of 1936 he and Mrs. Jordan went to Shelburne, Vermont, for rest and recuperation in the New England that was always dear to him. Here his condition became suddenly worse and he was removed to the hospital in Lewiston, Maine, where he died.

He had lived a full and active life and the honors that had come his way were many. He had been president of a number of organizations, including the Society of American Bacteriologists, the Epidemiological Society, the Chicago Pathological Society and the Institute of Medicine. He served as a member of the Board of Scientific Directors of the International Health Division of the Rockefeller Foundation, on the Medical Fellowship Board of the National Research Council, as a trustee of the McCormick Institute, a member of the Committee on Foods of the American Medical Association, as a consultant to the United States Public Health Service, as consulting bacteriologist to the Stream Pollution Laboratories of the Service and in many other capacities. A Fellow of the American Public Health Association, he was the recipient of its Sedgwick Medal in 1934. He was on the editorial boards of a number of scientific journals, was editor of the Journal of Preventive Medicine and joint

editor, with Hektoen, of the Journal of Infectious Diseases. He gave several endowed lectures, including the Gordon Bell Memorial Lectures, the Harvey Lectures and the Cutter Lectures. He was made an honorary Doctor of Science by the University of Cincinnati in 1921 and was elected a member of the National Academy of Sciences in 1936.

His students made up a very important part of his life. He knew no greater joy than that of developing to the best of his abilities the talents of promising young men and women and he followed their subsequent careers with keen personal interest. They, in turn, felt his warm and steady support and did not hesitate to call upon him for advice and encouragement in later years. He is said to have remarked upon one occasion that if he had done no productive research, he would still feel that his students alone would have made life well worth living.

Jordan's contributions to bacteriology, public health and preventive medicine in America can hardly be over-estimated. The leading American authority in the fields of his greatest interest, his influence was great and many of his contributions were of the subtle kind that escape general notice. His scientific acumen and the uncompromising probity with which he dealt with the problems he handled left a permanent impress on bacteriological thinking.

BIBLIOGRAPHY OF EDWIN OAKES JORDAN

1888

The numbers of bacteria in certain city tap waters. *Tech. Quar.*, vol. 2, pp. 322-324.

1889

A glass of water (with A. L. Kean). *Tech. Quar.*, Feb., pp. 229-234.

1890

A report on certain species of bacteria observed in sewage. *Mass. State Bd. Health Rept.* for 1888-89, Part II. Water-Supply and Sewage.

Investigations upon nitrification and the nitrifying organism (with Ellen H. Richards). *Mass. State Bd. Health Rept.* Part II.

Water-supply and sewage. Pp. 865-881.

Recent theories on the function of the white blood-cell. *Tech. Quar.*, vol. 3, pp. 170-181.

1891

The spermatophores of Diemyctylus. *Journ. Morph.*, vol. 5, pp. 263-270.

1893

The habits and development of the newt. Doctor's thesis, Clark University, May 11, 1892. *Journ. Morph.*, vol. 8, pp. 269-366.

1894

The identification of the typhoid fever bacillus. *Journ. Amer. Med. Assn.*, vol. 23, pp. 931-935.

On the cleavage of amphibian ova (with A. C. Eycleschymer). *Journ. Morph.*, vol. 9, pp. 407-416.

1895

On some conditions affecting the behavior of the typhoid bacillus in water. *Medical News*, Sept., 23 pp.

1896

The filtration of public water supplies. *Univ. Chicago Rec.*, vol. 2, pp. 430-431.

1898

The "inheritance" of certain bacterial diseases. *Chicago Med. Rec.*, vol. 15, pp. 82-86.

1899

The death-rate from diphtheria in the large cities of the United States. *Philadelphia Med. Journ.*, Feb., pp. 384-387.

Translation "The Principles of Bacteriology" by Ferdinand Hueppe. Chicago, pp. x, 467.

Bacillus pyocyaneus and its pigments. *Journ. Exper. Med.*, vol. 5, pp. 627-647.

The production of fluorescent pigment by bacteria. *Botan. Gaz.*, vol. 27, pp. 19-36.

The Chicago Drainage Canal. *Amer. Month. Rev. of Rev.* (before 1900), pp. 55-58.

1900

Notes on bacterial water analysis (with E. E. Irons). *Journ. Boston Soc. Med. Sci.,* vol. 4, pp. 81-82.

On certain species of bacteria observed in sewage. Experimental investigations of the State Board of Health of Massachusetts upon the purification of sewage and upon the intermittent filtration of water. Wright and Potter. Boston. 1890. Pp. 821-844.

St. Louis, Chicago, and the typhoid bacillus.

On the detection of Bacillus coli communis in water. *Journ. Boston Soc. Med. Sci.,* vol. 4, pp. 174-175.

Some observations upon the bacterial self-purification of streams. *Journn. Exper. Med.,* vol. 5, pp. 271-315.

1902

Notes on the occurrence and habitat of Anopheles punctipennis and Anopheles maculipennis. *Journ. Med. Res.,* vol. 7, N. S., vol. 2, pp. 1-24.

Typhoid fever and water supply in Chicago. *Journ. Amer. Med. Assn.,* vol. 39: 1561-1566.

On the nature of Pyocyanolysin. *Trans. Chicago Path. Soc.,* vol. 5, pp. 175-178.

1903

The kinds of bacteria found in river water. *Journ. Hyg.,* vol. 3, pp. 1-27.

The connection between the alkalinity of certain bacterial filtrates and their hemolytic power. *Journ. Med. Res.,* vol. 10, N. S., vol. 5, pp. 31-41.

The self-purification of streams. *Univ. Chicago Decennial Pubs.,* pp. 81-91.

The field of municipal hygiene. *Pop. Sci. Month.,* June, pp. 132-140.

The purification of water supplies by slow sand filtration. *Journ. Amer. Med. Assn.,* Oct. 3, 10, 17, 24, pp. 1-29.

Natural purification of streams. Paper presented at the 26th Annual Convention of the American Water Works Association, pp. 1-7.

1904

Analyses of Chicago market milk. Report of the Health and Sanitation Committee of the Civic Federation of Chicago, July, 19 pp.

The longevity of the typhoid bacillus in water (with H. L. Russell and F. R. Zeit). *Journ. Infec. Dis.,* vol. 1, pp. 641-689.

The sphere of bacteriology. *Amer. Med.,* vol. 8, pp. 875-879.

1905

Observations on the bionomics of Anopheles (with Mary Hefferan). *Journ. Infec. Dis.,* vol. 2, pp. 56-69.

A thermostabile, hemolytic precipitate from nutrient broth. *Journ. Infec. Dis.,* vol. 2, pp. 511-513.

Report on typhoid fever in Winnipeg, Manitoba. City Council, Winnipeg, Feb., pp. 1-19.

1906

Experiments with bacterial enzymes. Biological Studies by the pupils of William T. Sedgwick, Boston, pp. 123-145.

1907

The place of pathology in the university. *Journ. Amer. Med. Assn.*, vol. 48, pp. 917-919.

Testimony in the Chicago Drainage Case. *U. S. Geological Survey, Water Supply and Irrigation Paper, No.* 194. Pollution of Illinois and Mississippi Rivers by Chicago sewage. A digest of the testimony taken in the case of the State of Missouri vs. the State of Illinois and the Sanitary Canal District of Chicago, by M. O. Leighton. pp. 207-146.

1908

The problems of sanitation. *Journ. Amer. Med. Assn.*, vol. 50, pp. 493-498.

The cause of milksickness or trembles (with N. McL. Harris). *Journ. Amer. Med. Assn.*, vol. 50, pp. 1665-1673.

General bacteriology. Philadelphia. New Editions: 1911, 1913, 1915, 1917, 1919, 1922, 1925, 1929, 1932, 1935.

1909

Milksickness (with N. McL. Harris). *Journ. Infec. Dis.*, vol. 6, pp. 401-491.

1910

The school and the germ carrier. Proceedings of the First, Second and Third Conferences of the American School Hygiene Association, pp. 1-7.

Typhoid fever in Milwaukee and the water supply. *Journ. Amer. Med. Assn.*, vol. 55, pp. 211-215.

Typhoid fever in Detroit. *Journ. Amer. Med. Assn.*, vol. 55, pp. 1284-1287.

1911

Typhoid fever in Des Moines, Iowa. *Journ. Amer. Med. Assn.*, vol. 56, pp. 41-43.

Report of Committee on Regulations for the Pasteurization of Milk (with P. G. Heinemann). *Journ. Amer. Med. Assn.*, vol. 57, p. 975.

The campaign for pure milk. *Christendom*, pp. 485-488.

The household milk supply. *Pub. Domestic Sci. Dept., Univ. Chicago*, pp. 252-254.

Profitable and fruitless lines of endeavor in public health work. *Science*, N. S., vol. 33, pp. 833-839.

1912

The Rockford (Illinois) typhoid epidemic (with E. E. Irons). *Journ. Infec. Dis.*, vol. 11, pp. 21-43.

The case for pasteurization. *Journ. Amer. Med. Assn.*, vol. 59, pp. 1450-1457.

Three outbreaks of typhoid fever traced to milk infection (with E. E. Irons). *Journ. Amer. Med. Assn.*, vol. 58, pp. 169-172.

Typhoid fever in St. Charles. *Journ. Amer. Med. Assn.*, vol. 58, pp. 1941-1943.

1913

The municipal regulation of milk-supply. *Journ. Amer. Med. Assn.*, vol. 61, pp. 2286-2291.

The Quincy (Illinois) typhoid epidemic (with E. E. Irons). *Journ. Infec. Dis.*, vol. 13, pp. 16-29.

School diseases. *Journ. Amer. Med. Assn.*, vol. 60, pp. 409-411.

Disease carriers among school children. *Trans. Fourth International Congress on School Hygiene*, Aug., pp. 1-6. *Amer. Journ. Tropical Dis.*, vol. 1, pp. 220-226.

Typhoid fever in the large cities of the United States in 1912. *Journ. Amer. Med. Assn.*, May 31, p. 1702.

Ozone: its bactericidal, physiologic and deodorizing action (with A. J. Carlson). *Journ. Amer. Med. Assn.*, vol. 61, pp. 1007-1012.

The inhibitive action of bile upon B. coli. *Journ. Infec. Dis.*, vol. 12, pp. 326-334. *Trans. Chicago Path. Soc.*, vol. 9, p. 44.

1914

Typhoid in the large cities of the United States in 1913. *Journ. Amer. Med. Assn.*, May 9, p. 1473.

Bacteriological standard for drinking water. The standard adopted by the Treasury Department for drinking water supplied to the public by common carriers in interstate commerce. U. S. Public Health Reports, Reprint No. 232, vol. 29, pp. 2959-2966.

Typhoid fever. Council on Health and Medical Instruction of the American Medical Association, 1914.

Variation in bacteria. Read before the National Academy of Sciences, Dec. 9, 1914. *Proc. Nat. Acad. Sci.*, vol. 1 (1915), pp. 160-164.

1915

Municipal sanitation. Commemoration volume. Published by the American Medical Association, pp. 285-296.

Typhoid in the large cities of the United States in 1914. *Journ. Amer. Med. Assn.*, Apr. 17, p. 1322.

An infection with the paratyphoid bacillus (B. paratyphosus B) (with E. E. Irons). *Journ. Infec. Dis.*, vol. 16, pp. 234-240.

1916

Typhoid in the large cities of the United States in 1915. *Journ. Amer. Med. Assn.*, April 22, p. 1305.

The purification of water supplies. *Journ. Amer. Med. Assn.*, vol. 66, pp. 467-471.

1917

The differentiation of the paratyphoid-enteritidis group. I. *Journ. Infec. Dis.,* vol. 20, pp. 457-483.

The differentiation of the paratyphoid-enteritidis group. II. Lead acetate agar (with Ruth Victorson). *Journ. Infec. Dis.,* vol. 21, pp. 571-579.

The bacteriology of foods. Chairman's address, Society of American Bacteriologists, Dec. 29, 1916. *Journ. Amer. Med. Assn.,* vol. 68, pp. 1080-1084.

Food poisoning. Chicago, 115 pp.

Typhoid in the large cities of the United States in 1916. *Journ. Amer. Med. Assn.*

1918

Typhoid in the large cities of the United States in 1917. *Journ. Amer. Med. Assn.,* March 16, p. 777.

The differentiation of the paratyphoid-enteritidis group. III. The uncommonness of B. suipestifer in the intestines of normal swine. *Journ. Infec. Dis.,* vol. 22, pp. 252-257.

The differentiation of the paratyphoid-enteritidis group. IV. The behavior of B. paratyphosus A and B. Paratyphosus B in milk. *Journ. Infec. Dis.,* vol. 22, pp. 511-514.

The differentiation and distribution of the paratyphoid-enteritidis group. V. Occurrence in the human intestine (with E. E. Irons). *Journ. Infec. Dis.,* vol. 22, pp. 537-542.

Food-borne infections. Address of Vice-President and Chairman Section K, Physiology and Experimental Medicine, American Association for the Advancement of Science, December, 1917. *Science, N. S.,* vol. 47, pp. 80-86.

1919

The production of indol by certain strains of the Pfeiffer bacillus. (Influenza investigations, U. S. Public Health Service). *Journ. Amer. Med. Assn.,* vol. 72, pp. 1542-1543.

Influenza in three Chicago groups (with Dudley B. Reed and E. B. Fink). *Public Health Reports,* vol. 34, pp. 1528-1545.

Notes on the epidemiology of influenza. *Proc. Inst. Med.,* Chicago, vol. 2, pp. 135-141.

Observations on the bacteriology of influenza. *U. S. Public Health Reports,* vol. 34, 1413-1425; *Journ. Infec. Dis.,* vol. 25, pp. 28-40.

Typhoid in the large cities of the United States in 1918. *Journ. Amer. Med. Assn.,* vol. 72, April 5, p. 997.

1920

The differentiation of the paratyphoid-enteritidis group. VII. Irregular and variable strains. *Journ. Infec. Dis.,* vol. 26, pp. 427-434.

Influenza studies. I. Immunity in influenza (with W. B. Sharp). *Journ. Infec. Dis.,* vol. 26, pp. 463-468.

Typhoid in the large cities of the United States in 1919. *Journ. Amer. Med. Assn.*, March 6, p. 672.

1921

William Thompson Sedgwick. *Journ. Infec. Dis.*, vol. 28, pp. i-ii.

The relations of bacteriology to the public health movement since 1872. *Amer. Journ. Pub. Health*, vol. 11, pp. 1042-1047.

Influenza studies. IV. Effect of vaccination against influenza and some other respiratory infections (with W. B. Sharp). *Journ. Infec. Dis.*, vol. 28, pp. 357-365.

Typhoid in the large cities of the United States in 1920. *Journ. Amer. Med. Assn.*, March 26, p. 860.

1922

Influenza studies. X. The serologic relationships between strains of the Pfeiffer bacillus (with W. B. Sharp). *Journ. Infec. Dis.*, vol. 31, pp. 198-208.

Interepidemic influenza. *Amer. Journ. Hyg.*, vol. 2, pp. 325-345.

Typhoid in the large cities of the United States in 1921. *Journ. Amer. Med. Assn.*, March 25, p. 890.

1923

The differentiation of the paratyphoid-enteritidis group. VIII. Bacilli of the Paratyphosus B group. *Journ. Infec. Dis.*, vol. 33, pp. 567-575.

The "food poisoning" outbreaks apparently due to bacilli of the paratyphoid-enteritidis group (with J. C. Geiger). *Journ. Infec. Dis.*, vol. 32, pp. 471-478.

Influenza studies. XIV. The common cold (with John F. Norton and W. B. Sharp). *Journ. Infec. Dis.*, vol. 33, pp. 416-433.

Studies on respiratory diseases. XII. Epidemiology of colds in infants (with W. F. Winholt). *Journ. Amer. Med. Assn.*, vol. 81, pp. 280-282.

Typhoid in the large cities of the United States in 1922. *Journ. Amer. Med. Assn.*, vol. 80, pp. 691-694.

1924

Studies on respiratory diseases. XVI. Further observations on the characteristics and occurrence of the Hemophilic bacteria (with A. F. Reith). *Journ. Infec. Dis.*, vol. 34, pp. 239-259.

Influenza studies. XVII. Agglutinins for Pfeiffer bacillus in serum of influenza and measles patients (with W. B. Sharp). *Journ. Infec. Dis.*, vol. 34, pp. 305-311.

William Thompson Sedgwick. A pioneer of public health. (With S. C. Whipple and C.-E. A. Winslow. Introduction by Mary K. Sedgwick.) New Haven, 193 pp.

The effect of Cl. sporogenes on Cl. botulinum (with G. M. Dack). *Journ. Infec. Dis.*, vol. 35, pp. 576-580.

Typhoid in the large cities of the United States in 1923. *Journ. Amer. Med. Assn.*, vol. 82, pp. 389-391.

Diphtheria mortality in the large cities of the United States. *Journ. Amer. Med. Assn.*, vol. 83, pp. 918-923.

1925

The viability of typhoid bacilli in shell oysters. *Journ. Amer. Med. Assn.*, vol. 84, pp. 1402-1403.

The differentiation of the paratyphoid-enteritidis group. IX. Strains from various mammalian hosts. *Journ. Infec. Dis.*, vol. 36, pp. 309-329.

Typhoid in the large cities of the United States in 1924. *Journ. Amer. Med. Assn.*, vol. 84, pp. 813-815.

Diphtheria mortality in the large cities of the United States. *Journ. Amer. Med. Assn.*, vol. 84, pp. 1269-1271.

Typhoid in certain large cities of Europe in 1924. *Journ. Amer. Med. Assn.*, vol. 85, pp. 1890-1891.

1926

The present status of the influenza problem. *Amer. Journ. Pub. Health*, (1925), pp. 943-947; also *The Medical Officer*, Jan. 16.

Standard methods of milk analysis. Bacteriological methods. *Amer. Journ. Pub. Health*, August, pp. 811-818. (Report of the referee for the bacteriologic examination of milk.)

The changes in the bacterial content of stored normal and typhoid feces. *Journ. Infec. Dis.*, vol. 38, pp. 306-322.

The interconvertibility of "rough" and "smooth" bacterial types. *Journ. Amer. Med. Assn.*, vol. 86, pp. 177-178.

Typhoid in the large cities of the United States in 1925. *Journ. Amer. Med. Assn.*, vol. 86, pp. 948-950.

Diphtheria mortality in large cities of the United States in 1925. *Journ. Amer. Med. Assn.*, vol. 86, pp. 1005-1007.

Diphtheria in certain large cities of Europe in 1924. *Journ. Amer. Med. Assn.*, vol. 86, p. 194.

1927

The Gordon Bell Memorial Lecture on Food Poisoning. Given under the auspices of the Winnipeg Medical Society, April 22. *Pub. Health Journ.*, December (1927), and January (1928), pp. 1-21.

Epidemic influenza. Chicago. *A. M. A. Press*, pp. 599.

The relation of paratyphoid bacilli to food poisoning. *Amer. Journ. Pub. Health*, vol. 17, pp. 1221-1225.

The pasteurization of milk. *Journ. Amer. Med. Assn.*, editorial (after 1927).

Typhoid in the large cities of the United States in 1926. *Journ. Amer. Med. Assn.*, April 9, p. 1148.

Diphtheria mortality in large cities of the United States in 1926. *Journ. Amer. Med. Assn.*, vol. 88, pp. 1396-1398.

1928

The bacteria of food poisoning. Chapter from The Newer Knowledge of Bacteriology and Immunology, E. O. Jordan and I. S. Falk, editors, Chicago, pp. x, 1196; pp. 443-451.

A new differential medium for the paratyphoid group (with Paul H. Harmon). *Journ. Infec. Dis.*, vol. 42, pp. 238-241.

Typhoid in the large cities of the United States in 1927. *Journ. Amer. Med. Assn.*, vol. 90, pp. 1624-1627.

Diphtheria mortality in large cities of the United States in 1927. *Journ. Amer. Med. Assn.*, vol. 90, pp. 1621-1624.

1929

"Food poisoning" produced in monkeys by feeding living Salmonella cultures (with G. M. Dack and W. L. Wood). *Journ. Preventive Med.*, vol. 3, pp. 153-158.

The epidemiology of paratyphoid infections. Cutter Lecture. *Journ. Preventive Med.*, vol. 3, pp. 279-307.

Experimental "food-poisoning" in monkeys with living paratyphoid bacilli, (with G. M. Dack and W. L. Wood). *Proc. Soc. Exper. Biol. and Med.*, vol. 26, pp. 301-302.

Food poisoning. Paper presented at the School of Tropical Medicine, San Juan, Puerto Rico, embodying portions of the Gordon Bell Lecture. *Puerto Rico Review of Public Health and Tropical Medicine*, vol. 4, pp. 517-537.

Typhoid in the large cities of the United States in 1928. *Journ. Amer. Med. Assn.*, vol. 92, pp. 1672-1674.

Diphtheria mortality in large cities of the United States in 1928. *Journ. Amer. Med. Assn.*, vol. 92, pp. 1759-1762.

1930

The production by staphylococci of a substance causing food poisoning. *Journ. Amer. Med. Assn.*, vol. 94, pp. 1648-1650.

The role of staphylococci in food poisoning. *Proc. Soc. Exper. Biol. and Med.*, vol. 27, pp. 741-742.

Typhoid in the large cities of the United States in 1929. *Journ. Amer. Med. Assn.*, vol. 94, pp. 1574-1576.

Diphtheria mortality in large cities of the United States in 1929. *Journ. Amer. Med. Assn.*, vol. 94, pp. 1838-1841.

1931

Attempts to immunize human volunteers with staphylococcus filtrates that are toxic to man when swallowed (with G. M. Dack and O. C. Woolpert). *Journ. Preventive Med.*, vol. 5, pp. 151-159.

Results of feeding staphylococcus filtrates to monkeys (with Josephine McBroom). *Proc. Soc. Exper. Biol. and Med.*, vol. 29, pp. 161-162.

The effect of heat, storage and chlorination on the toxicity of staphylococcus filtrates (with G. M. Dack and O. C. Woolpert). *Journ. Preventive Med.*, vol. 5, pp. 383-386.

Food poisoning and food-borne infection. Chicago. Pp. xi, 286.

University of Chicago, Division of Biological Sciences, Department of Hygiene and Bacteriology, *Methods and Problems of Medical Education.* 19th Series. Rockefeller Foundation. New York.

A case of food poisoning apparently due to staphylococcus (with John R. Hall). *Journ. Preventive Med.,* vol. 5, pp. 387-389.

Staphylococcus food poisoning. *Journ. Amer. Med. Assn.,* vol. 97, pp. 1704-1707.

Typhoid in the large cities of the United States in 1930. *Journ. Amer. Med. Assn.,* vol. 96, pp. 1576-1579.

Diphtheria mortality in large cities of the United States in 1930. *Journ. Amer. Med. Assn.,* vol. 96, pp. 1768-1771.

1932

Some unsolved problems in epidemiology. President's address, December 6. *Proc. Inst. Med. of Chicago,* vol. 9, pp. 185-198.

The occurrence of Brucella agglutinins in cattle in the Panama Canal Zone (with Josephine McBroom). *Journ. Amer. Veterinary Med. Assn.,* vol. 81, N. S., vol. 34, pp. 401-404.

Typhoid in the large cities of the United States in 1931. *Journ. Amer. Med. Assn.,* vol. 98, pp. 1550-1552.

Diphtheria mortality in large cities of the United States in 1931. *Journ. Amer. Med. Assn.,* vol. 98, pp. 1644-1646.

1933

Influence of age on amount of normal agglutinins in the blood of cattle. *Proc. Soc. Exper. Biol. and Med.,* vol. 30, pp. 446-447.

Impressions of health conditions in Jamaica. Personal communication to the Medical Directors of the Rockefeller Foundation (mimeographed). No. 7778.

Nature of the substance causing staphylococcus food poisoning (with W. Burrows). *Proc. Soc. Exper. Biol. and Med.,* vol. 30, pp. 448-449.

A Great Opportunity. 173rd Convocation Address, University of Chicago, August 25. *University Record,* vol. 19, pp. 225-230.

Typhoid in the large cities of the United States in 1932. *Journ. Amer. Med. Assn.,* vol. 100, pp. 1491-1494.

Diphtheria mortality in large cities of the United States in 1932. *Journ. Amer. Med. Assn.,* vol. 100, pp. 1595-1597.

1934

Essentials of typhoid fever control today. *Amer. Journ. of Pub. Health,* vol. 24, pp. 349-354.

Notes on intestinal flora in the tropics (with Josephine McBroom). *Amer. Journ. Trop. Med.,* vol. 14, pp. 27-32.

An outbreak of food poisoning apparently caused by a new serologic type of Salmonella (S. panama). *Journ. Infec. Dis.,* vol. 55, pp. 224-227.

Akee poisoning (with W. Burrows). *Proc. Soc. Exper. Biol. and Med.,* vol. 31, pp. 515-517.

Further observations on staphylococcus food poisoning (with W. Burrows). *Amer. Journ. Hyg.*, vol. 20, pp. 604-610.

Bacterial motility (with Mary E. Caldwell and Dorothy Reiter). *Journ. Bacter.*, vol. 27, pp. 165-174.

Streptococcus food poisoning (with W. Burrows). *Journ. Infec. Dis.*, vol. 55, pp. 363-367.

Typhoid in the large cities of the United States in 1933. *Journ. Amer. Med. Assn.*, vol. 102, pp. 1677-1680.

Diphtheria mortality in large cities of the United States in 1933. *Journ. Amer. Med. Assn.*, vol. 102, pp. 1758-1760.

1935

The Morgan bacillus (with R. R. Crawford and Josephine McBroom). *Journ. Bacter.*, vol. 29, pp. 131-148.

The production of enterotoxic substance by bacteria (with W. Burrows). *Journ. Infec. Dis.*, vol. 57, pp. 121-128.

Is a special variety of staphylococcus concerned in food poisoning? (with J. Stritar). *Journ. Infec. Dis.*, vol. 56, pp. 1-7.

Typhoid in the large cities of the United States in 1934. *Journ. Amer. Med. Assn.*, vol. 104, pp. 2093-2096.

Diphtheria mortality in large cities of the United States in 1934. *Journ. Amer. Med. Assn.*, vol. 104, pp. 2182-2186.

Oxidation-reduction potentials in Salmonella cultures. I. The development of potential levels characteristic of species (with W. Burrows). *Journ. Infec. Dis.*, vol. 56, pp. 255-263.

1936

Note on the liquefaction of gelatin by Salmonella, type Dar-es-Salaam. *Journ. Infec. Dis.*, vol. 58, p. 128.

Oxidation-reduction potentials in Salmonella cultures. II. Characteristic potentials produced by members of the suipestifer and enteritidis groups (with W. Burrows). *Journ. Infec. Dis.*, vol. 58, pp. 259-262.

Food poisoning. *Journ. Amer. Med. Assn.*, vol. 107, pp. 1720-1721.

Water filtration vs. chlorination. Editorial in *Journ. Amer. Med. Assn.*, vol. 107, p. 1474.

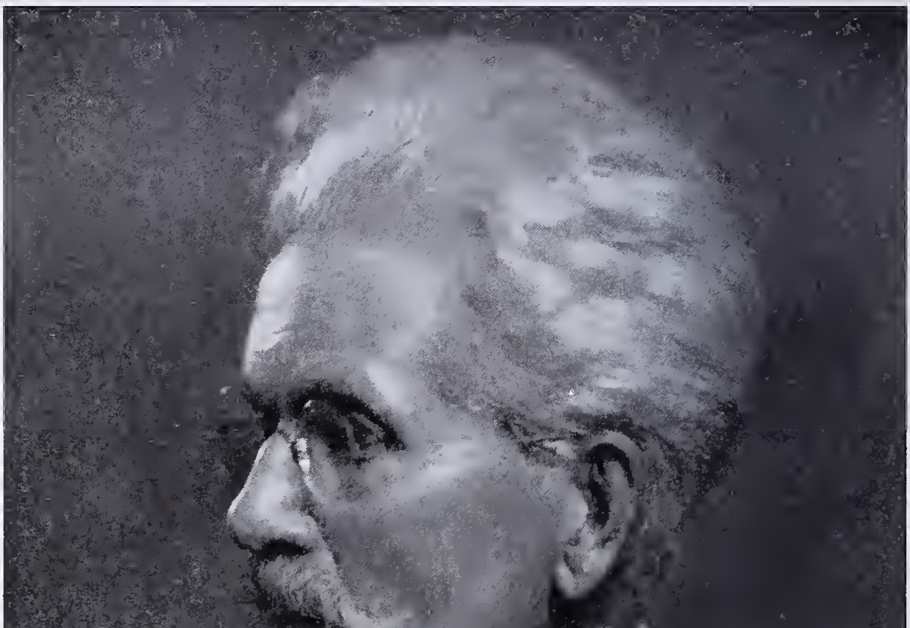
Typhoid in the large cities of the United States in 1935. *Journ. Amer. Med. Assn.*, vol. 106, pp. 1983-1986.

Diphtheria mortality in large cities of the United States in 1935. *Journ. Amer. Med. Assn.*, vol. 106, pp. 2060-2063.

1937

The vomiting sickness of Jamaica B. W. I. and its relations to akee poisoning (with W. Burrows). *Amer. Journ. Hyg.*, vol. 25, pp. 520-545.

Normal bacterial agglutinins and their significance. *Journ. Infec. Dis.*, vol. 61, pp. 79-87.



Henry H. Donaldson

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OF

HENRY HERBERT DONALDSON

1857–1938

BY

EDWIN G. CONKLIN

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

HENRY HERBERT DONALDSON

1857–1938

BY EDWIN G. CONKLIN

Henry Herbert Donaldson was the son of John Joseph and Louisa Goddard (McGowan) Donaldson. Both his parents were of Irish stock though both were born on this side of the Atlantic; his father was a native of New York where he was a successful banker; his mother was born in Montreal, Canada. She was a handsome woman, noted for her orderliness and great presence of mind, in which qualities her son resembled her. Both his parents lived to an advanced age, his father dying at 79 and his mother at 84; his grandparents also were equally long lived. Their children were Henry Herbert, born at Yonkers, New York, May 12, 1857, and Alfred Lee, born in 1866. The latter like his father was a banker; he was also the author of a "History of the Adirondacks" and was something of a poet and musician. The elder son prepared for college at Phillips Academy, Andover, Massachusetts; he then entered Yale and was graduated in 1879 with the degree of Bachelor of Arts. His father had desired that his son should join him in business but yielding to the young man's preference for science it was agreed that he should enter the medical profession. In further preparation for medicine he spent an additional year at Yale working with Professor Russell H. Chittenden on the detection of arsenic in various organs of the body in cases of arsenical poisoning. The results of this year's work were published jointly with his professor as his first scientific paper. (See appended bibliography.)

During the year 1880–81 he attended the College of Physicians and Surgeons in New York but realizing that his interests were in research rather than medicine he accepted a fellowship in biology in the graduate school of Johns Hopkins University in 1881, which appointment he held for two years, specializing in physiology under Professor H. Newell Martin. As a result of this work he published four papers on physiology and pharmacology, two of them under joint authorship with other students. (See bibliography.)

After his two years as fellow he was appointed student assistant in the department of biology for the year 1883–84. At the close of this academic year he married Julia Desboro Vaux of New York and he and his bride spent the summer of 1884 at Beaufort, North Carolina, in Professor Brooks' laboratory. During the following year he finished his thesis for the doctor's degree under the direction of G. Stanley Hall who was at that time professor of psychology at Johns Hopkins, and was awarded the Ph.D. in 1885. His thesis was entitled "On the temperature sense" and concerned the mapping of heat-sensitive and cold-sensitive areas of the skin. During the following year he was Professor Hall's assistant and they completed and published jointly a paper on "Motor sensations of the skin".

His work on the sensory areas of the skin led him to seek more extensive training in neurology in European centers, and in February 1886 he and his wife and his father went abroad and remained abroad until the autumn of 1887. During their year and a half abroad he worked in Forel's laboratory at Zürich and von Gudden's at Munich and spent some time with Meynert at Vienna and Golgi at Pavia. At the close of this European visit he returned to Baltimore as associate in psychology at the Johns Hopkins University and this position he held from 1887 to 1889.

During his years at Johns Hopkins he was associated or acquainted with a group of unusually able and stimulating fellow students in biology. William T. Sedgwick and Edmund B. Wilson had received the Ph.D. degree the year he entered but K. Mitsukuri, H. W. Conn, Frederick S. Lee and J. Playfair McMurrich were fellow students. In his second year as fellow (1882–83) J. McKeen Cattell and William H. Howell were also fellows in biology, and the following year Adam Bruce and Ethan A. Andrews were fellows in biology, while fellows in other departments included Henry Crew, John Dewey, Woodrow Wilson and other men of brilliant mind who later attained wide distinction. This stimulating fellowship undoubtedly had a great influence on young Donaldson.

In 1888 G. Stanley Hall became president and professor of psychology in the newly founded Clark University at Worcester,

Mass., and in 1889 Doctor Donaldson was called to Clark as assistant professor of neurology. His proven ability in this field led President Hall to assign to him for study the brain of Laura Bridgman, a blind deaf mute who had been taught to speak and had attained marked mental ability. Parts of this long and detailed study were published in 1891 and 1892 and it has been characterized as "probably the most thorough study of a single human brain that has ever been carried out". Of this study Dr. Donaldson has written in a brief autobiographical note: "The chief modifications found in this brain were caused by an arrest of growth due to the destruction of the sense organs. This made it desirable to know the developmental state of the brain at the time of the destructive illness (two years). Such information was not in the literature. With the hope of contributing to fill this gap I arranged a program for the study of the brain (nervous system) from birth to maturity. In carrying out this plan quantitative methods were used and data on the size and weight of the parts and on the number of cells in them were especially considered."

This led him to that long, accurate, quantitative study of growth which was the main theme of his life work. He gathered together all the available material on the growth of the central nervous system and published it in a book entitled, "The growth of the brain: a study of the nervous system in relation to education" (Scribner, 1895).

He remained at Clark University until 1892 when he joined the migration of many of the faculty to the reorganized University of Chicago, where he became professor of neurology and was very active in the development of the scientific departments. From 1892 to 1898 he served as dean of the Ogden Graduate School of Science of the University of Chicago. During this time a tubercular infection of the hip joint interrupted his work and left him permanently lame, but after a prolonged visit to Colorado he returned to his work with courage and determination and from 1898 until his death there was never a year when he did not publish one or more researches.

In 1905 ten professors of anatomy and zoology in leading universities were invited to serve as the Scientific Advisory Board of the Wistar Institute of Anatomy and Biology in Philadelphia. Among these were Charles S. Minot of Harvard, George S. Huntington of Columbia, Franklin P. Mall of Johns Hopkins, George A. Piersol and Edwin G. Conklin of the University of Pennsylvania, Simon H. Gage of Cornell, G. Carl Huber and J. Playfair McMurrich (later at Toronto) of the University of Michigan, Lewellys F. Barker of Chicago (later at Johns Hopkins) and Henry H. Donaldson of Chicago. On the invitation of the founder of the Institute, General Isaac J. Wistar, and of its Director, Dr. Milton J. Greenman, this Advisory Board met at the Institute in April, 1905 and was asked to propose a plan for the future development of the Institute. It was the unanimous opinion of the Board that the Institute should be devoted primarily to research and in the beginning to research in neurology. This met with the hearty approval of General Wistar, and the Board was asked to recommend some one to organize this work. Dr. Donaldson was the unanimous choice of all the other members and after long and serious consideration he accepted the appointment, and in the following year transferred his activities and his chief assistant, Dr. S. Hatai, from the University of Chicago to the Wistar Institute where he became professor of neurology and director of research.

While at Chicago Donaldson had published one book and seventeen papers, most of them on the human nervous system and that of the frog. However, his attention was called to the peculiar advantages of the albino rat as a laboratory animal in 1893 when Dr. Adolph Meyer had used the rat in a course on the anatomy of the nervous system. One of Dr. Donaldson's associates, Dr. Hatai, had published fifteen papers based on the white rat before removing to Philadelphia. This work on the rat convinced Donaldson that it was the best available mammal for laboratory work on problems of growth. In an autobiographical note he says: "I selected the albino rat as the animal with which to work. It was found that the nervous system of the rat grows in the same manner as that of

man—only some thirty times as fast. Further, the rat of three years may be regarded as equivalent in age to a man of ninety years, and this equivalence holds through all portions of the span of life, from birth to maturity. By the use of the equivalent ages observations on the nervous system of the rat can be transferred to man and tested. The results so obtained show a satisfactory agreement and indicate that the rat may be used for further studies in this field."

For accurate quantitative studies of growth it was necessary to establish a standard stock and to get rid as far as possible of individual differences caused by peculiarities of heredity or environment. Accordingly he and his associates set about the problem of producing a pure bred stock raised under accurately controlled conditions which would give a standardized strain of laboratory animals. How well they succeeded in this is recognized throughout the world by the wide use in the most accurate work of the "Wistar Institute stock" of white rats.

This work on the standardization of a research mammal was summarized in 1915 in a book entitled, "The Rat: Reference tables and data for the albino rat and the Norway rat" (Memoir of the Wistar Institute). A revised and enlarged edition of this book was published in 1924. It may be said that this work renders a service in mammalian research comparable to that of pure chemicals in chemical research or to the "Tables of Physical Constants" in the physical sciences. Largely because of this work the albino rat has become the most widely used laboratory mammal.

The major theme running through the whole of Donaldson's work was organic growth. In his presidential address before the Association of American Anatomists in 1916-17 he said: "Were I asked to name some direction in which we might extend our work I should naturally lay weight upon post-natal growth in the terms of cell multiplication and cell structure, with its many subsidiary problems." Much of this work was on the nervous system, but it was extended to include muscles, viscera, skeleton and teeth, both in normal and in experimental conditions. Several papers were on the number of nerve cells and fibers, others on the size of these, and many papers were

devoted to the effects of domestication, exercise, feeding, castration and age on the size and weight of particular organs. The determinations of the percentage of water in the central nervous system under all of the experimental conditions named constitute a large section of his work.

Altogether he personally published nearly one hundred papers and monographs on these subjects, and his students, assistants and associates published more than three hundred and sixty separate articles on these and related subjects. His method of directing the research work of associates is well described in his published report to the Wistar Advisory Board in 1925 (pp. 46-48): "No investigator is ever asked to do anything which is not for his individual and scientific welfare. For the most part those who come to the Institute are in the early stages of their scientific work and do not bring their problems with them. It is for us, therefore, to suggest one. . . . As an aid in obtaining orderly data which will interlock, the custom of dealing with papers used by the distinguished physiologist Carl Ludwig has been followed. The papers of the younger men have been in every case read critically by some member of the staff familiar with the field, with the new observations brought into relation with those previously published from the Institute. Such criticism assists the younger writer in several ways and also makes it possible to tie together the results of consecutive studies in a manner that gives them cumulative value.—In every case the investigators receive full personal credit for their work. This is as it should be, for it is the virture of academic laboratories that the emphasis is put on the individual rather than on the institution."

Among the many fellow workers who were associated with him at the University of Chicago and the Wistar Institute the following should receive especial mention: Irving Hardesty, S. Hatai, Alice Hamilton, John B. Watson, S. W. Ranson, W. H. F. Addison, Ezra Allen, A. W. Angulo, G. E. Coghill, Eunice C. Greene, Frederick S. Hammett, Helen Dean King, W. and M. C. Koch. In addition to these some forty other investigators were associated with him in his work.

His interest in and sympathy with all types of good scien-

tific work were broad and generous. He had keen appreciation of good literature, music and the graphic arts. His concern with social problems and human welfare was deep and genuine, and even his closest friends never learned from him of his generous contributions and acts of kindness to those who were in need. His students and colleagues knew him as a man of infinite patience, even temper and nobility of character and they loved and honored him. Among those who published work done in his laboratory were some thirty Americans, twenty Japanese and a smaller number of other nationalities; many of these persons are leaders in their professions and all of them revere his memory.

For his eminence in research he received the honorary degree of Sc. D. from Yale in 1906 and from Clark University in 1937. He was elected president of the Association of American Anatomists for 1916-18, of the American Society of Naturalists in 1927 and of the American Neurological Association in 1937. He was elected to membership in the American Philosophical Society in 1906 and was a Councillor of the Society for four terms of three years each, namely, 1911-13, 1915-17, 1928-31, 1932-36; he was chairman of the Publication Committee from 1932 until his death and was instrumental in establishing the new series of *Memoirs* of the Society, and from 1935 until his death he was a vice-president of the Society. He was a member of the Corporation of the Marine Biological Laboratory from its foundation in 1888 until his death and a Trustee from 1912 to 1929 when he became Trustee Emeritus. In 1911 he established his summer home at Woods Hole, Massachusetts and every summer thereafter, with the exception of two when he was abroad, he carried on his research work there at the Marine Biological Laboratory. He was elected to membership in the National Academy of Sciences in 1914 and was a member of the Council in 1919. He was also an honored member of ten other American and foreign scientific societies. For twenty years he was president of the Lenape Club of the University of Pennsylvania and on the occasion of his eightieth birthday in 1937 a bronze portrait medallion of him, made by Dr. R. Tait McKenzie, was placed in the Club

with appropriate ceremonies and a replica of the medallion now hangs in the hall of the American Philosophical Society.

On the seventy-fifth anniversary of his birth, May 12, 1932, a special anniversary volume of the *Journal of Comparative Neurology* was dedicated to him. It was preceded by an admirable portrait and contained a brief sketch of his distinguished career, followed by twenty scientific contributions from associates and friends and the following affectionate testimonial and address:

"Professor Donaldson's long and productive career is an illustrious example of rigid adherence to a well planned program of research on a fundamental theme without wavering or diversion by opportunistic considerations. He is internationally known as a worthy representative of the best traditions of American science and he has won the respect of the scientific world for his consistent and fruitful program of research.

"He has won the esteem and affection of the Editorial Board by unfailing courtesy, loyal friendship and generous support of all worthy enterprises. For his cordial and invaluable cooperation and wise council during nearly thirty-five years the *Journal of Comparative Neurology* owes him much.

"Professor Donaldson: We your colleagues on this anniversary offer our congratulations upon your past achievements, and we rejoice with you in the realization that your productiveness in research and in the wider field of human relationships continues in full vigor and efficiency. We know, too, that the universities and other organizations which you have so ably served in the past and all the numberless friends who have the good fortune to know you personally wish to join with us in this expression of appreciation."

His personal appearance was so distinguished that it always commanded attention and admiration. Any one who had once seen him could never forget his magnificent head, his steady sympathetic eyes, his gentle smile. With these were associated great-hearted kindness, transparent sincerity, genial humor. Perhaps his most distinctive personal characteristic was the quality which Sir William Osler celebrates in his essay, "*Equinimitas.*" With this were naturally associated orderli-

ness, persistance, serenity. His laboratory and library were always in perfect order, his comings and goings were as timely as the clock, he never seemed hurried and yet he worked "Ohne Hast, ohne Rast."

In 1884 he married Julia Desboro Vaux of New York, who died in 1904. Two children were born to them, John C. Donaldson, now professor of anatomy in the Medical School of the University of Pittsburgh and Norman V. Donaldson at present secretary of the Yale University Press. In 1907 he married Emma Brace of New York and their hospitable homes in Philadelphia and Woods Hole were known to a host of loving friends.

After his long illness in the middle nineties of the last century he was never in robust health, but was almost never incapacitated for his regular work. Until a few days before his death he was at work as usual in his laboratory at the Institute. His end came as a result of pneumonia and heart failure at his home in Philadelphia on January 23, 1938, in his eighty-first year. With characteristic foresight he and Mrs. Donaldson had sometime before planned the simple and appropriate funeral service which should be held in the event of either's death. His pallbearers were chosen from among his scientific associates and the officers of the Institute, the University of Pennsylvania and the American Philosophical Society. In accordance with the terms of his will his brain was preserved and added to the notable collection at the Wistar Institute and his body was cremated. His work, influence and memory remain to make the world richer for his having lived in it.

BIBLIOGRAPHY OF HENRY H. DONALDSON

On the detection and determination of arsenic in organic matter. (By R. H. Chittenden and H. H. Donaldson). Am. Chem. J., vol. 2, no. 4, 1881.

Influence of digitaline on the work done by the heart of the "Slider" terrapin (Pseudemys rugosa, Shaw). (With M. Warfield). Trans. Medico-Chirurg. Fac. of State of Maryland, 84th Ann. Session, Baltimore, April, 1882.

The influence of digitaline on the work of the heart and on the flow through the blood-vessels. (With F. L. Stevens). J. Physiol., vol. 4, pp. 165-197, 1883.

Action of the muriate of cocaine on the temperature nerves. Maryland Med. J., April 18, 1885.

On the temperature sense. (Ph.D. Thesis). Mind, vol. 10, no. 39, 1885.

Motor sensations of the skin. (With G. Stanley Hall). Mind, no. 40, pp. 557-572, 1886.

Animal heat. Woods Reference Handbook of the Medical Sciences, vol. 3, 1886.

On the relation of neurology to psychology. Am. J. Psychol., February, 1888.

Notes on the models of the brain. Am. J. Psychol., vol. 4, no. 1, 1891.

Anatomical observations on the brain and several sense organs of the blind deaf mute, Laura Dewey Bridgman. Am. J. Psychol., vol. 3, pp. 293-342 and vol. 4, pp. 248-294, 1891.

The size of several cranial nerves in man as indicated by the areas of their cross-sections. (With T. L. Bolton). Am. J. Psychol., vol. 4, pp. 224-229, 1891.

The extent of the visual cortex in man, as deduced from the study of Laura Bridgman's brain. Am. J. Psychol., vol. 4, pp. 503-513, 1892.

Preliminary observations on some changes caused in the nervous tissues by reagents commonly employed to harden them. J. Morph., vol. 9, no. 1, pp. 123-166, 1894.

The growth of the brain: A study of the nervous system in relation to education. Charles Scribner's Sons, New York, 1895.

The central nervous system. Am. Text Book of Physiol., vol. 2, no. 4, pp. 171-297, 1898.

Observations on the weight and length of the central nervous system and of the legs in bullfrogs of different sizes. J. Comp. Neur., vol. 8, no. 4, pp. 314-335, 1898.

A note on the significance of the small volume of the nerve cell bodies in the cerebral cortex in man. J. Comp. Neur., vol. 9, no. 2, pp. 141-149, 1899.

Observations on the weight and length of the central nervous system, and of the legs in frogs of different sizes (Rana virescens brachycephala,

Cope). (With D. M. Schoemaker). J. Comp. Neur., vol. 10, no. 1, pp. 109-132, 1900.

The functional significance of the size and shape of the neurone. J. Nerv. and Ment. Diseases, vol, 27, no. 10, 1900.

Growth of the brain. Woods Reference Handbook of the Medical Sciences, vol. 2, 1901.

Review of "Atlas of the nervous system, including an epitome of the anatomy, pathology and treatment" by Christfried Jacob. With a preface by Ad. v. Strümpell. Psychol. Review, vol. 8, no. 6, pp. 622-626, 1900.

Observations on the post-mortem absorption of water by the spinal cord of the frog (Rana virescens). (With D. M. Schoemaker). J. Comp. Neur., vol. 12, pp. 183-198, 1902.

On a formula for determining the weight of the central nervous system of the frog from the weight and length of the entire body. Decennial Pub., Univ. of Chicago, vol. 10, pp. 3-15, 1902.

A description of charts showing the areas of the cross-sections of the human spinal cord at the level of each spinal nerve. (With D. J. Davis). J. Comp. Neur., vol. 13, pp. 19-40, 1903.

On a law determining the number of medullated nerve fibers innervating the thigh, shank and foot of the frog (Rana virescens). J. Comp. Neur., vol. 13, pp. 223-257, 1903.

On the areas of the axis cylinder and medullary sheath as seen in cross-sections of the spinal nerves of vertebrates. (With G. W. Hoke). J. Comp. Neur., vol. 15, pp. 1-16, 1905.

Problems in human anatomy. Science, vol. 21, pp. 16-26, 1905.

Some aspects of the endowment of research. Science, vol. 23, no. 582, pp. 282-286, February 23, 1906.

A comparison of the white rat with man in respect to the growth of the entire body. Boas Anniversary Volume, pp. 5-26, 1906.

Rana pipiens. Science, N.S., vol. 26, no. 655, p. 78, July 19, 1907.

Document of the Report of the President of the Brain Commission (Br. C.) by Waldeyer. Translated from the German by Henry H. Donaldson. J. Comp. Neur., vol. 18, no. 1, pp. 87-90, 1908.

Cooperation in biological research. Science, N.S., vol. 27, no. 688, pp. 375-377, March 6, 1908.

Review of "Die Grosshirnrinde des Menschen in ihren Massen und in ihrem Fasergahalt," by Dr. Theodor Kaes. Anat. Rec. (no. 8) in Am. J. Anat., vol. 7, no. 4, pp. 242-251, 1908.

The nervous system of the American leopard frog, Rana pipiens, compared with that of the European frog, Rana esculenta and Rana temporaria (fusca). J. Comp. Neur., vol. 18, no. 2, pp. 121-149, 1908.

A comparison of the albino rat with man in respect to the growth of the brain and of the spinal cord. J. Comp. Neur., vol. 18, no. 4, pp. 345-392, 1908.

Review of "The Problem of Age, Growth and Death," by Charles S. Minot. Anat. Rec., vol. 2, no. 8, pp. 372-377, 1908.

Translation of the Report of the Executive Committee of the Brain Commission. Anat. Rec., vol. 2, no. 9, pp. 428-431, 1908.

On the relation of the body length to the body weight and to the weight of the brain and of the spinal cord in the albino rat (Mus norvegicus var. albus). J. Comp. Neur., vol. 19, no. 2, pp. 155-167, 1909.

Further observations on the nervous system of the American leopard frog (Rana pipiens) compared with that of the European frogs (Rana esculenta and Rana temporaria). J. Comp. Neur., vol. 20, no. 1, pp. 1-18, 1910.

On the percentage of water in the brain and in the spinal cord of the albino rat. J. Comp. Neur., vol. 20, no. 2, pp. 119-144, 1910.

On the influence of exercise on the weight of the central nervous system of the albino rat. J. Comp. Neur., vol. 21, no. 2, pp. 129-137, 1911.

The effect of underfeeding on the percentage of water, on the ether-alcohol extract, and on medullation in the central nervous system of the albino rat. J. Comp. Neur., vol. 21, no. 2, pp. 139-145, 1911.

An interpretation of some differences in the percentage of water found in the central nervous system of the albino rat and due to conditions other than age. J. Comp. Neur., vol. 21, no. 2, pp. 161-176, 1911.

Note on the influence of castration on the weight of the brain and spinal cord in the albino rat and on the percentage of water in them. (With S. Hatai.) J. Comp. Neur., vol. 21, no. 2, pp. 155-160, 1911.

Studies on the growth of the mammalian nervous system. Philadelphia Neurological Society, President's Address. J. Nerv. and Ment. Dis., vol. 38, no. 5, pp. 257-266, 1911.

On the regular seasonal changes in the relative weight of the central nervous system of the leopard frog. J. Morph., vol. 22, no. 3, pp. 663-694, 1911.

A comparison of the Norway rat with the albino rat in respect to body length, brain weight, spinal cord weight and the percentage of water in both the brain and the spinal cord. (With S. Hatai.) J. Comp. Neur., vol. 21, no. 5, pp. 417-458, 1911.

A comparison of the European Norway and albino rats (Mus norvegicus and Mus norvegicus albinus) with those of North America in respect to the weight of the central nervous system and to cranial capacity. J. Comp. Neur., vol. 22, no. 1, pp. 71-97, 1912.

On the weight of the crania of Norway and albino rats from three stations in western Europe and one station in the United States. Anat. Rec., vol. 6, no. 2, pp. 53-63, 1912.

Review of "Localization Motrice et Kinesthesique," by Edouard Willems. J. Nerv. and Ment. Dis., vol. 39, no. 1, pp. 67-72, 1912.

Research foundations in their relation to medicine. Science, N.S., vol. 36, no. 916, pp. 65-74, July 19, 1912.

The history and zoological position of the albino rat. J. Acad. Nat. Sciences of Phila., vol. 15, 2nd series, pp. 365-369, 1912.

An anatomical analysis of growth. Trans. of the 15th Internat. Congress on Hygiene and Demography, Wash., D. C., Sept. 23-28, 1912.

The mutual relations of medical progress and the physician. Science, N.S., vol. 38, no. 969, pp. 101-109, July 25, 1913.

The growth of the brain. Woods Reference Handbook of the Medical Sciences, 3rd ed., pp. 387-395, 1913.

Charles Sedgwick Minot. December 23, 1852-November 19, 1914. Science, N.S., vol. 40, no. 1043, pp. 926-927, December 25, 1914.

Charles Sedgwick Minot. Including Dr. Charles W. Eliot's address on Dr. Minot. Proc. of the Boston Soc. of Nat. Hist., vol. 35, pp. 79-93, 1915.

The Rat. Reference tables and data for the albino rat (Mus norvegicus albinus) and the Norway rat (Mus norvegicus). Mem. Wistar Inst. of Anat. and Biol., no. 6, pp. 1-278, 1915.

Postnatal growth of the brain under several experimental conditions. Studies on the albino rat. (With S. Hatai and H. D. King.) J. Nerv. and Ment. Disease, vol. 42, pp. 797-801, 1915.

The relation of myelin to the loss of water in the mammalian nervous system with advancing age. Proc., Nat. Acad. Sci., vol. 2, pp. 350-356, 1916.

A preliminary determination of the part played by myelin in reducing the water content of the mammalian nervous system (albino rat). J. Comp. Neur., vol. 26, pp. 443-451, 1916.

Herrick's "An Introduction to Neurology." J. Animal Behavior, vol. 6, pp. 426-432, 1916.

A revision of the percentage of water in the brain and in the spinal cord of the albino rat. J. Comp. Neur., vol. 27, pp. 77-115, 1916.

Experiment on the feralization of the albino rat. Year Book No. 15, Carnegie Inst. Washington (for 1916), pp. 200-201, 1916.

Biological problems and the American Association of Anatomists. Anat. Rec., vol. 11, pp. 299-309, 1917.

More complete titles. Science, N.S., vol. 45, no. 1156, p. 190, February 23, 1917.

Growth changes in the mammalian nervous system. Harvey Lectures, series 12, 1916-1917, pp. 133-150. J. B. Lippincott Co., Philadelphia, 1917.

Introduction to the "Comparative studies on the growth of the cerebral cortex," by Naoki Sugita. Parts I and II in J. Comp. Neur., vol. 28, no. 3, December, 1917; parts III to VIII, vol. 29, nos. 1 to 3, February, April and June, 1918.

On the increase in the diameters of nerve-cell bodies and of the fibers arising from them during the later phases of growth (albino rat). (With G. Nagasaka.) J. Comp. Neur., vol. 29, pp. 529-552, 1918.

A comparison of growth changes in the nervous system of the rat with

corresponding changes in the nervous system of man. Proc. Nat. Acad. Sci., vol. 4, pp. 280-283, 1918.

Medicine and growth. Science, N.S., vol. 49, no. 1266, pp. 315-322, April 4, 1919.

Quantitative studies on the growth of the skeleton of the albino rat. (With Sara B. Conrow.) Am. J. Anat., vol. 26, pp. 237-314, 1919.

Report on the studies of Dr. R. S. Ellis touching the structural changes in the human cerebellum from birth to old age. Trans., 46th Annual Meeting Am. Neur. Assoc., New York, June 1-3, 1920.

On the changes in the relative weights of the viscera and other organs from birth to maturity—albino rat. Am. J. Physiol., vol. 67, pp. 1-21, 1923.

On the cranial capacity of the guinea pig—wild and domesticated. Archives Suisses de Neur. et de Psych., vol. 13, pp. 206-222, 1923.

Naming and exact naming. Science, vol. 59, p. 554, June 20, 1924.

The Rat: Data and reference tables. Second edition, revised and enlarged. American Anatomical Memoir no. 6, 8 vo., xiv + 469 pp., 212 tables, 72 charts, 13 figures, with bibliography comprising 2329 titles, 1924.

The significance of brain weight. Archives of Neur. and Psych., vol. 13, pp. 385-386, 1925.

On the control of the rat population. Science, vol. 61, pp. 305-306, 1925.

Research at the Wistar Institute, 1905-1925. Bull., no. 6, pp. 43-51. Celebration of the 20th Anniversary of the organization of the Advisory Board, 1925.

Review of "Anatomy of the Wood Rat," by A. Brazier Howell. Monographs of the American Society of Mammalogists. Anat. Rec., vol. 34, no. 3, pp. 219-220, December 25, 1926.

On the growth in the diameters of the molar teeth of the albino rat, after eruption. (With Harley E. French.) Anat. Rec., vol. 34, no. 5, pp. 277-299, February 25, 1927.

A study of the brains of three scholars: Granville Stanley Hall, Sir William Osler, and Edward Sylvester Morse. (With M. M. Canavan.) J. Comp. Neur., vol. 46, no. 1, pp. 1-95, August 15, 1928.

Life processes and size of the body and organs of the gray Norway rat during ten generations in captivity. I. Life processes. II. Size of the body and organs. (By Helen Dean King and H. H. Donaldson.) Am. Anat. Memoir, no. 14, February, 1929.

On the growth of the molar teeth after eruption in the albino rat and in the cat. J. Dental Research, vol. 9, no. 3, pp. 285-287, 1929.

On the weight of the parts of the brain and on the percentage of water in them according to brain weight and to age, in albino and in wild Norway rats. (With Shinkishi Hatai.) J. Comp. Neur., vol. 53, no. 2, pp. 263-307, 1931.

Note on the contour of the foramen magnum in different strains of the Norway rat. Anat. Rec., vol. 52, no. 4, pp. 313-320, 1932.

The early days of the American Physiological Society. Science, vol. 75, no. 1954, pp. 599-601, 1932.

On the effects of exercise carried through seven generations on the weight of the musculature and on the composition and weight of several organs of the albino rat. (With Ruth Elva Meeser.) Am. J. Anat., vol. 50, no. 3, pp. 359-396, 1932.

The brain problem—in relation to weight and form. Am. J. Psychiatry, vol. 12, no. 2, pp. 197-214, 1932.

On the effect of exercise beginning at different ages on the weight of the musculature and of several organs of the albino rat. (With Ruth E. Meeser.) Am. J. Anat., vol. 53, no. 3, pp. 403-411, 1933.

On the increase in the diameters of the molar teeth of the rat after eruption. J. Dental Research, vol. 14, no. 5, pp. 323-346, 1934.

Effect of prolonged rest following exercise on the weights of the organs of the albino rat. (With Ruth E. Meeser.) Am. J. Anat., vol. 56, no. 1, pp. 45-55, 1935.

Summary of data for the effects of exercise on the organ weights of the albino rat: comparison with similar data from the dog. Am. J. Anat., vol. 56, no. 1, 1935.

On the percentage of water in the organs of albino rats after prolonged exercise. Am. J. Anat., vol. 59, pp. 55-62, 1936.

On the growth of the eye in three strains of the Norway rat. (With Helen Dean King.) Am. J. Anat., vol. 60, pp. 203-229, 1937.

The Nervous Skeleton. Presidential address before the American Neurological Association, Atlantic City, June 3, 1937. Trans., Amer. Neurological Assn. 1938.

Vernon Kellogg

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OF

VERNON LYMAN KELLOGG

1867–1937

BY

C. E. McCLUNG

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

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Dates, places and events are easy to record and in the life of a man have their significance, but his nature is not revealed thereby. When the time comes to make a record of the real character of one of our friends, we are always oppressed with the inadequacy of our understanding and the feebleness of our expression. David Starr Jordan with two large volumes tried, in "The Days of a Man," to show what it was that made him the man he became. His friend and disciple, Vernon Lyman Kellogg, whose contribution to Jordan's development is so fully recorded therein, leaves for himself no such delineation of character. It remains for us who knew him to do what we can to estimate and record his life and achievements. Such a brief notice as this, however, can serve no further purpose than to anticipate a fuller record which doubtless will be forthcoming.

To the University of Kansas, in the translated New England town of Lawrence, came the young Kellogg in 1885. Here, in this infant institution, he was thrown intimately into contact, as student, assistant and secretary, with scholarly, New England-trained Francis H. Snow, the newly elected Chancellor and an enthusiastic entomologist. They became firm friends and the association proved profitable in every way to Kellogg.

The beginning of his work at Kansas University is characteristic of his whole career. Always the opportunity for the next step—always the ability to utilize it. These opportunities were many and varied and called for a wide range of qualifications—social, scientific, linguistic, humanitarian. But underlying all these qualities and making their application easy and effective was a personality so pleasing, adaptive, persuasive and charming that opposition usually failed to develop. No matter what the type of work that engaged Kellogg's attention, the course of events was much the same. In part this facility of operation resulted from his habit of inconspicuousness. In most of his endeavors he associated himself with some strong and outstanding personality which occupied the public eye and took the

blame or the credit for what resulted. Kellogg meanwhile pursued his studied course, fully aware of the practical bearings of his policies and of the reactions which they awakened. With infinite tact and an uncanny appreciation of personalities, he quietly pursued his way, arousing little opposition and creating no ill feeling. If opposition developed and proved obdurate he did not stress his position, but put the matter aside until a more favorable opportunity. In the event that delay did not improve his chance of success, he made no further effort.

The years at Lawrence were busy ones, but he had time to make many friends—Chancellor Snow, an enthusiast of indefatigable vigor; S. W. Williston, a man of profound understanding and high scientific ideals—an authority in such diverse fields as vertebrate paleontology and dipterology; E. C. Franklin, later a colleague at Stanford, a man of high ideals and achievements; and E. E. Slosson, a writer on scientific subjects of unusual literary ability. The University was young and in the formative stage, with no hindering traditions. Kellogg was free to go his own way and took full advantage of the opportunity. This way led him to activities beyond the campus limits. He wrote for the local paper a column on birds and this led to reportorial work and even to editorial efforts. Thus his urge to write, which later became engrossing, early manifested itself. A desire to travel, which was not so easily satisfied in those days, led to frequent trips to Colorado and elsewhere. Human interest in all his occupations was prominent, whether in efforts to attract people to the study of birds, to protect them from the attacks of injurious insects, or to show them the possible influence of biology in human life. All in all, these years sketched, in broad outline, Kellogg's future course—administration, writing, investigating, interpreting and teaching.

While it is true that the influence of the college years was direct and strong, it is also true that the qualities which they revealed in Kellogg were inherent and had already evidenced themselves in his early youth. William Allen White, who knew him well, in an editorial in the *Emporia Gazette,* gives a charming description of these early days:

"His was a happy boyhood. It was busy and purposeful. It foreshadowed his life. Few boys who have grown up in this town have got so much out of the first years as he did.

"They lived such lives as boys now know only in envious dreams. They skated and swam, trapped and hunted and fished and studied wild life until the whole annual panorama of nature with the going and coming of plants and birds and flowers and the passing colors of the grass and trees became a part of their life.

"Is it a wonder that such a boy became a scientist? How could he help it? When he left this town to go to the University of Kansas in 1885 at 18, his fate was written inexorably in the blood and environment of childhood. A college professor's son, Vernon had learned casually to love the outer manifestations of nature. He yearned secretly to study the inner sources of things."

The few years of experience at the University of Kansas were so fruitful and revealing that they brought Kellogg to the notice of President Jordan, and without hesitation, he offered Kellogg a position on the faculty of Stanford University. Here he came intimately into contact with President Jordan, just as he had with Chancellor Snow at Kansas, and with him he collaborated in teaching and writing. The association was stimulating and helpful in many ways both to Kellogg and to Jordan; fruitful to the University—and to its students.

Here the greatest amount of Kellogg's scientific writing was done, and here he practically ended his career as a teacher and investigator. In view of his great local influence it is curious to note how few were his contacts with fellow biologists in their organizations, and how slight the recognition of his excellent work. This is probably due to the fact that he placed emphasis upon the popularization of biology rather than upon its extension. And yet his scientific bibliography alone would do great credit to any investigator. His sustained interest over many years in the Mallophaga made him the leading authority in this group. But this taxonomic work was only incidental to the question of the evolutionary importance of the biting lice. This same phylogenetic interest he carried over into the study of other insect groups. There was, in his mind, always the broad significance of the biological facts he had discovered. Even the extensive experimental work on the silk worm, extending over a

period of fifteen years, traced back to this interest. The character and extent of his writings are so well revealed in his bibliography that they need not be mentioned further.

The years at Stanford stand out as those of his greatest scientific productivity. A constant stream of books, reviews, addresses and research papers came year after year from his pen, evidencing sustained interest and power. At the same time he became influential in the affairs of the University and an inspiration to its students. With Jordan he gave a course on evolution which aroused such interest and enthusiasm that numerous study groups were formed for more extended discussions. Here Kellogg grew and ripened and prepared himself unknowingly for the heavy responsibilities that were later to fall on his shoulders.

The final scenes of Kellogg's life were laid in places remote from those of his early life, and they were remote also in the character of the interests which they held. These interests were much more general and popular and their relations and implications more generally understood. The activities which they engendered made but small contribution to the development of his personality and character, which were well established when the world's madness called him to Europe to make application of the knowledge and training which the west had given him. Circumstances which he there encountered did, however, open up much greater opportunities for his talents, and doubtless greatly strengthened and broadened his purpose to make biology a force in human affairs. His success in alleviating suffering and in interpreting the motives and activities of contending peoples is well known and is evident in the honors that came to him.

Participation of the United States in the world war brought him at last to Washington to aid in the organization of science in support of the Government. The first formal result of these efforts was the organization of the National Research Council in which he became chairman of the Divisions of Agriculture, Botany and Zoology. When the Council was later made a continuing body he was named the Permanent Secretary, in which office he continued, active and emeritus, until his death.

He was also Chairman of the Division of Educational Relations for ten years and a member of innumerable committees. Indeed he was the real integrating, and largely directive, force in the operation of this body which has done so much to make the scientific organizations of this country a working force. His conduct in all the important matters which came up for action was characterized by tolerance, good judgment and practical idealism. It is not too much to say that whatever success the Council has had is due largely to his activities. The limited space available makes a detailed enumeration of his achievements impossible, but they may be inferred from the list of offices he occupied.

If one were required to designate the most outstanding characteristic of Kellogg, he would unhesitatingly think of his intense and sustained activity—both mental and physical. His mind was constantly thrusting out in search of new ideas and contacts. This led him early into research—an interest which he maintained throughout his life, although in later years his response to more insistent demands did not permit its continuation. But always the pace was too slow when it depended upon the efforts of one individual and so he read much and widely. A considerable proportion of his bibliography is occupied with titles of reviews and critiques. This accumulation of information led to the production of numerous text books, and many newspaper and magazine articles. As his experience broadened the subjects of his discussions became less and less technical and more and more general. Practical applications of biology always interested him, but as the years passed and he saw more clearly the service which biology might render to social progress, this became the theme of his writings. To conceive a thought was to express it. His judgment, nevertheless, was remarkably good for one who wrote so readily and continuously. In time the ethical implications of scientific thought came to occupy much of his attention. As an example of this phase of his thinking an excerpt from his discussion of death may be given: "Death may possibly be not only that normal incident in human life we recognize it to be, but it may be simply one, the last one we now know, of a series of profound evolutionary changes in an

organism which has a continuing career of which we know now only the earlier stages; that is the stages of conception, embryology, adolescence, senescence and death.

"Death may not be the end, but simply another change in human life, greater and more radical, but perhaps no less possible than the changes from the single egg cell to myriad-celled and utterly different. Death may be but the change from one condition of humanness to another."

Instead of relying entirely upon my own judgment for a choice of the qualities in Kellogg which were most characteristic and significant, I consulted the opinions of others. Some of these are here recorded:

Mme. Jusserand: "I think of his splendid work, of his modesty and disinterestedness, of his eagerness to help his fellow men by his science and learning. And how could I ever forget what we owe him, here in France, for the lives he saved and for the sympathy he showed for our people in their hours of dire need. His intelligence, his heart, his tact made him succeed in a task where the lack of either would have meant failure."

Harold Heath: "From the outset he displayed a keenness of intellect, and a most active interest in literary and educational subjects, as well as in his chosen field, biology. * * * The interest created by these past masters (Jordan and Kellogg) in the art of presentation was great indeed. Discussion groups were formed in the student body and it is safe to say that the results exerted a profound influence upon many individuals and schools far beyond the confines of the Stanford campus. * * * It is safe to say that he exercised a lasting influence on the early life of Stanford University and was one of its great leaders."

Harlan Stone, Supreme Court of the United States: "To those of us who knew Vernon Kellogg best, his life presents a pattern of contrasts which are nevertheless singularly harmonious. * * * Scientifically trained, for most of his life a teacher of science, and never forsaking his scientific interest, he became more and more the guide, philosopher and friend of worthwhile educational and philanthropic undertakings.

"The eminent service which he rendered to science, to education, and the humanities, and above all the grace and integrity of

his mind, revealed to the stranger by his gifted pen, are the precious memories of those who knew him, and who, knowing, loved him."

Ray Lyman Wilbur: "He was characterized by a beautiful clearness and simplicity in thinking and statement. This combined with his appreciation of art, his dramatic sense and his broad human sympathy and understanding, made him one of our great authors of popular science."

Resolutions of the Academic Council, Stanford University: " * * * these words spoken in grateful remembrance of a charming friend, distinguished colleague, great-hearted and far-seeing citizen of the world."

Editorial, Washington Post: "Dr. Vernon Kellogg was one of those rare spirits, found most frequently in the scientific world, in whom unusual talent and unusual charm of character were most happily combined. * * * The man who is honored alike by scientists, by statesmen, and by little children is one whose contribution will endure. * * * So the name of Vernon Kellogg rests secure among those Americans of our day who have been of memorable service to humanity."

Editorial, Emporia Gazette: "But what he learned in journalism, indeed what he learned anywhere, he took with him. His life was an accumulation of ten thousand things that he had learned in passing through the wilderness of the world. So he was gentle, wise and kind to the end. * * * With all his learning, with all his wisdom, with all his gentleness, and with all the love he bore so many friends, also he had great courage."

CHRONOLOGY

1867. Born December 1, 1867, Emporia, Kansas, son of Lyman Beecher and Abigail (Homer) Kellogg.
1889. Graduated, A.B., University of Kansas.
1890-93. Assistant Professor of Entomology, University of Kansas.
1892. Graduate, M.S., University of Kansas.
1893. Student, University of Leipzig.
1893-94. Associate Professor of Entomology, University of Kansas.
1894-95. Assistant Professor of Entomology, Leland Stanford University.
1895-96. Associate Professor of Entomology, Leland Stanford University.
1896-1920. Professor of Entomology, Leland Stanford University.
1897. University of Leipzig.

1904. University of Paris.
1908. At Florence, Italy, married Charlotte Hoffman of Berkeley, Calif.
1908. University of Paris.
1910. His daughter, Jean Kellogg, born in Berkeley, California.
1915-16. Director, Brussels, American Committee for Relief of the Belgians.
1917-19. Assistant to U. S. Food Administrator.
1918. Chairman, Division of Agriculture, National Research Council.
1918-21. Chief of mission to Poland, special investigator in Russia, member American Relief Administration.
1919-31. Permanent Secretary of the National Research Council.
1919-29. Chairman of Division of Educational Relations, National Research Council.
1919-34. Member of Research Information Service, National Research Council.
1919-34. Member, Division of States Relations, National Research Council.
1919-33. Member, Division of Foreign Relations, National Research Council; Vice-chairman, 1921-33.
1921-33. Board of Trustees, Science Service.
1925-31. Member of Executive Committee of International Research Council.
1931. Secretary Emeritus, National Research Council.
1937. Died, August 8, at Hartford, Connecticut.

DEGREES AND MEMBERSHIP IN SOCIETIES

LL.D. University of California, 1919, Brown, 1920; Sc.D. Oberlin, 1922.

National Academy of Sciences.

American Society of Naturalists; American Entomological Society; Ecological Society; Association of Economic Entomologists; Genetics Association; American Philosophical Society; Washington Academy; Kansas Academy; California Academy; Academy of Natural Sciences, Philadelphia; Entomologische Gesellschaft; Société Entomologique de France.

Officer of the Legion of Honor (France) Commander of the Crown (Belgium) Commander of the Order of Leopold I (Belgium) Commander of the Order of Polonia Restituta (Poland), Gold medal (Poland).

Trustees of Rockefeller Foundation, Brookings Institution, Gallaudet College.

BIBLIOGRAPHY

The extensive series of publications by Kellogg is impossible to list in its entirety in the limited space here available. Therefore only books and the more important scientific articles will be mentioned by name, while the total numbers of other classes of writings will be given. The range of subjects treated is most astonishing and rarely, even in hastily written articles, is there any lapse in style or scientific accuracy. When it is remembered that this extensive series of writings is but the by-product of a life full of teaching and administration, its extent and character are almost unbelievable.

In addition to the list of scientific papers, books and articles in books, here appended, there appeared book reviews to the number of 37 from 1920 to 1924; magazine articles to the number of 102 from 1916 to 1926; and newspaper articles syndicated, in many papers, to the number of 52 during the years 1920 to 1927.

EARLY OR MORE IMPORTANT ARTICLES

1895

The classification of the Lepidoptera. Am. Naturalist 5:248-257.

The mouthparts of the Lepidoptera. Am. Naturalist 5:546-556.

The affinities of the Lepidopterous Wing. Am. Naturalist 5:709-717.

(With F. J. Jack.) The California Phryganidian (Phryganidia californica Pack). Proc. Calif. Acad. Sci. ser. 2., 5: 562-570. Sept.

The Ephemeridae and venation nomenclature. Psyche 7:311-315. December.

1896

New Mallophaga. I. Proc. Calif. Acad. Sci. ser. 2, 6:31-168. March.

The Mallophaga. Psyche 7:375-379. May.

New Mallophaga. II. Proc. Calif. Acad. Sci. ser. 2, 6:431-548.

Mallophaga of North American Birds. Zool. Anz. 19:121-123.

1899

Mallophaga from birds of Panama, Baja, California and Alaska. Occ. Papers, Calif. Acad. Sci. 6:1-52. February.

(With B. L. Chapman.) Mallophaga from birds of California. Occ. Papers, Calif. Acad. Sci. 6:53-141. February.

The mouthparts of the Nematocerous Diptera I-V. Psyche 8:303-306, 327-330, 346-348, 355-359, 363-365, January-June.

A list of the Biting lice (Mallophaga) taken from birds and mammals of North America. Proc. U. S. Nat. Mus. 22:39-100.

1900

Notes on the Structure and Life History of Blepharocera capitata Loew. Ent. News 11:305-318. January.

(With S. I. Kuwana.) Mallophaga from Alaska birds. Proc. Phila. Acad. Sci. 23:151-159.

1901

Phagocytosis in the Post-embryonic Development of the Diptera. Am. Naturalist 35:363-368. May.

1902

(With B. L. Chapman.) Mallophaga from birds of the Pacific coast of North America. Jour. N. Y. Ent. Soc. 10:20-28.

Development and Homologies of the Mouthparts of Insects. Am. Naturalist 36:683-706. September.

1903

The Net-winged Midges (Blepharoceridae) of North America. Proc. Calif. Acad. Sci. 3d ser. 3:187-232. February.

Two New Genera of Mallophaga. Biol. Bull. 5:85-91. July.

Some Insect Reflexes. Science 18:693-696. November.

1904

Restorative Regeneration, in Nature, of the Star-fish, Linckia diplax (Muller and Troschel). Jour. Exp. Zool. 1:353-356. August.

(With R. G. Bell.) Variations induced in larval, pupal and imaginal stages of Bombyx mori by controlled varying food supply. Science 18: 741-748. December.

(With R. G. Bell.) Studies of variation in insects. Proc. Wash. Acad. Sci. 6:203-332. December.

Regeneration in larval eggs of silkworms. Jour. Exp. Zool. 1:593-599. 10 figs. December.

Influence of primary reproductive organs on secondary sexual characters. Jour. Exp. Zool. 1:601-605. December.

1906

Physiological regeneration in insects. Science 23:149-152. January.

Galls and gall flies. Nature Study Review 2: 109-114. March.

Mallophaga from Argentina. Journ. N. Y. Ent. Soc. 14:45-49. March.

Colors of butterflies and moths. Nature Study Review 2:206-211. September.

The scientific aspects of Luther Burbank's works. Popular Science Monthly 69:363-374. October.

Is there determinate variation? Science 24:621-628. November.

Variation in parthenogenetic insects. Science 24:695-699. November.

A second collection of Mallophaga from birds of the Galapagos and Revillagigedo Islands and neighboring waters. *In* Trans. Am. Ent. Soc. 32:315-324. November.

1907

Sex differentiation in larval insects. Biol. Bull. 12:380-384.

Artificial parthenogenesis in the silkworm. Biol. Bull. 14:15-22. December.

1908

The Mallophaga of the world. Systematic summary. Psyche 15:11-13. February.
(With R. G. Smith.) Inheritance in silkworms. I. University Series, Leland Stanford Jr. University Pubs. 89 pp. 4 pl.
The Mallophaga of the Kilimandjaro Region. Wiss. Ergeb. Schwed. Deutsch.-Afrika Exp. Fasc. 15:43-56. 1 pl.

1910

Is there determinate variation? Science 31:401-403. November.

1911

An experiment in double mating. Science 33:783-789. May.
(With J. H. Paine.) Anoplura and Mallophaga from African Hosts. Bull. Entom. Res. 2:145-152. July.

1914

Mallophaga from birds of the South Atlantic. Sci. Bull. Brooklyn Inst. Art and Sci. 2:80-89. November.
The bionomics of war. Social Hygiene 1:44-52. December.

BOOKS

(With J. H. Comstock.) The elements of insect anatomy. 91 pp. 1895. Comstock Publishing Co., Ithaca, New York.
(With D. S. Jordan.) Animal Life. 329 pp. 180 figs. 1900. Appleton, New York.
(With O. P. Jenkins.) Lessons in nature study. 191 pp. 82 figs. 1900. Whitaker and Ray, San Franciso.
Elementary zoology. 492 pp. 172 figs. 1901. Henry Holt and Co., New York.
—— —— 2nd ed. revised. 484 pp. 172 figs. 1902. Henry Holt and Co., New York.
First lessons in zoology. 363 pp. 257 figs. 1903. Henry Holt and Co., New York.
(With D. S. Jordan.) Animal studies (composed of selected chapters from "Animal Life," Jordan and Kellogg; "Animal Forms," Jordan and Heath; and new chapters). 459 pp. 254 figs. 1903. Appleton, New York.
(With J. H. Comstock.) The elements of insect anatomy. 5th ed. 145 pp. 1904. Comstock Publishing Co., Ithaca, New York.
American insects. 647 pp. 812 figs. 1905. Henry Holt and Co., New York.
Darwinism today. 403 pp. 1907. Henry Holt and Co., New York.
(With D. S. Jordan.) Evolution and animal life. 489 pp. 298 figs. 1907. D. Appleton and Co., New York.

Diptera, Family Blepharoceridae. Fasc. 56, Genera Insectorum, 15 pp. 1907. P. Wytsman, Brussels.

American insects, 2d ed. revised. 694 pp. 812 figs. 1908. Henry Holt and Co., New York.

Insect stories. 298 pp. 1908. Henry Holt and Co., New York.

Internal anatomy of Diptera. *In* Williston's "North American Diptera." pp. 49-51. 1908. Jas. T. Hathaway, New Haven.

In and out of Florence: A new introduction to a well known city. By Max Vernon (pseud.). 370 pp. 1910. Henry Holt and Co., New York.

Luigi Bertelli's "Clondolino" trans. by S. F. Woodruff, under the title "The Prince and his Ants." Edited with introduction by V. L. Kellogg, 1910. Henry Holt and Co., New York.

(With I. McCracken.) The animals and man: An elementary text-book of zoology and human physiology. 495 pp. 244 figs. 1911. Henry Holt and Co., New York.

Beyond war. 172 pp. 1912. Henry Holt and Co., New York.

(With R. W. Doane.) Economic zoology and entomology. 532 pp. 245 figs. March, 1915. Henry Holt and Co., New York.

Military selection and race deterioration. (Monograph with C. Bodart on Losses of Life in Modern Wars.) 206 pp. 1916. Carnegie Endowment for International Peace. Clarendon Press, Oxford.

Headquarters nights. 116 pp. 1917. The Atlantic Monthly Press, Boston.

(With A. E. Taylor.) The food problem. 213 pp. 1917. The Macmillan Co., New York.

Fighting starvation in Belgium. 219 pp. 21 illus. 1918. Doubleday Page and Co., New York.

Germany in the war and after. 101 pp. 1919. The Macmillan Co., New York.

Herbert Hoover, the man and his work. 376 pp. 1920. D. Appleton and Co., New York.

Nuova, the new bee. 150 pp. 15 illus. 1920. Houghton Mifflin Co., New York.

Human life as the biologist sees it. 140 pp. 1922. Henry Holt and Co., New York.

Mind and heredity. 108 pp. 1923. The Princeton Univ. Press, Princeton, N. J.

Evolution. 285 pp. 1924. D. Appleton and Co., New York.

Reading with a purpose—Biology. 40 pp. May, 1925. American Library Association, Chicago.

CHAPTERS OR ARTICLES IN BOOKS

Eugenics and Militarism. pp. 220-231, in "Problems of Eugenics," being the papers read before the First International Eugenics Congress, London, July, 1912. Eugenics Education Society, London.

All of the articles, 183 in number, on insects, in "The New Practical Reference Library," 1915. Hanson-Bellows Co., Chicago, Illinois.
The World Food Problem (Introduction pp. 7-9) in "The Abingdon War-Food Book." 58 pp. 1918. The Abingdon Press, New York.
Introduction. pp. xi-xiv, (with Herbert Hoover and Frederic C. Walcott) in "The Future of German Industrial Exports" by S. Herzog. 196 pp. 1918. Doubleday Page and Co., New York.
The Food Problem. pp. 265-276 in "The New World of Science," edited by Robert M. Yerkes. 443 pp. 1930. The Century Co., New York.
The University and Research. In "The University and the Commonwealth." 1921. University of Minnesota Press.
Insect Sociology. pp. 199-211 in "Science Remaking the World," edited by Otis Caldwell and Edwin E. Slosson. 292 pp. 1923. Doubleday Page and Co., New York.
Scientific Research. pp. 349-353 in "An Outline of Careers," edited by E. L. Bernays. 431 pp. 1927. George H. Doran Co., New York.

F. A. P. Barnard.

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OF

FREDERICK AUGUSTUS PORTER BARNARD

1809–1889

BY

CHARLES B. DAVENPORT

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

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1809–1889

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There have been enough cases of brothers and fathers and sons elected to the National Academy of Sciences to support the view of hereditary genius. Besides Agassiz, Bailey, Dana, Draper, Lyman, Mayer (Mayor), Mendenhall, Silliman, Van Vleck, fathers and sons; there have been elected the brothers Hilgard, LeConte, Whitney and Compton. One of the striking cases is that of the Barnard brothers, John G. Barnard, one of the incorporators of the Academy,* and Frederick Augustus Porter Barnard also an incorporator, subject of the present memoir. John was engineer and mathematician; Frederick administrator and mathematician.

These brothers are stars of a famous galaxy. Their father, Robert F. Barnard of Sheffield, Massachusetts, was a lawyer of marked ability who was several times state senator. His father in turn was a physician, and a generation or two back we have military men and a physician. The mother of these brothers was Augusta Porter. Through her side of the house there were half cousins (1) Henry Porter Andrews (b. 1822) who was a civil engineer attached to the Engineer Corps, U. S. A., and helped John G. Barnard in his survey of the isthmus of Tehuantepec. He helped fortify the "Golden Gate" and New York harbor defenses, and was paymaster of the army throughout the Civil War. Also, (2) Henry C. Walton, graduated from Columbia School of Mines, a metallurgist of distinction.

* Of John Gross Barnard the Academy has published a biography. Born 1815, he studied at West Point, graduated second in a class of 43. He erected fortifications at Pensacola and New Orleans, was principal engineer in war with Mexico, superintended the defense at Tampico and battlefields about Mexico. He was chief of the survey of the isthmus of Tehuantepec for a route to the Pacific ocean; Superintendent of U. S. Military Academy 1855-56, and a leading engineer in the army during the Civil War.

His biographer states that he was "modest and retiring in disposition, considerate and courteous, warm in his sympathies, and his name will be cherished with peculiar love and affection by his brother officers. He had a keen sense of humor and a passionate love of music."

Of the brothers of the mother's father Joshua Porter was a surgeon in the Continental Army and first president of Saratoga Springs village; his nephew was a state senator in Connecticut. A brother of Joshua, Augustus Porter, was a land surveyor and had a son who became United States Senator. Another brother of Joshua was a Member of Congress before the war of 1812, became brevet major general in the war, and was Secretary of War in President Madison's cabinet. His son, Captain Peter, was killed in action in the Civil War. Others of this galaxy might be cited who were distinguished chemists, metallurgists and warriors.

Into a less desirable heritage the Barnards were born. There was hardness of hearing (otosclerosis) on the mother's side. She was affected and both of her sons.

Such was the family background of Frederick, born at Sheffield, Massachusetts, on May 5, 1809. His education was somewhat incidental till he attended Saratoga Academy while living with his mother's father at Saratoga Springs. After further schooling at Stockbridge he entered Yale College, 1824, and was graduated in 1828, standing second in the honor list.

After two years of teaching at Hartford, Frederick was called to Yale College as tutor. At that time each sub-senior class was divided into groups each of which recited all lessons to one tutor. Barnard inaugurated the reform of having tutors for each special subject. His was mathematics.

Growing deafness led him to accept a tutorship (May, 1831) at a Hartford school for deaf mutes and a year later at the New York Institution for the Deaf and Dumb. While at the latter institution he published an Analytical Grammar (1836) intended for the deaf, and prepared a paper on the aurora. In 1837 he was elected professor of mathematics and natural philosophy at the University of Alabama, and continued there for seventeen years, the last six as professor of chemistry. In 1854 he was called to the chair of astronomy and mathematics at the University of Mississippi at Oxford. Two years later he was elected to the office of President, later changed to Chancellor. At Alabama he built and furnished a small astronomical observatory and suspended a Foucault pendulum from a dome by a ninety-

foot piano wire. He invented stereoscopic photography. He took up journalism, sometimes writing political editorials for the two newspapers of opposed principles, and on occasion refuting his own editorials. He served as a commissioner to relocate the boundary between Florida and Alabama. He took a leading part in discussions as to university policy. In 1854 he received orders in the Episcopal church.

At Oxford, Barnard built up a strong institution, secured the erection of astronomical and magnetic observatories and ordered a nineteen-inch lens from Clark of Cambridge; but as this was not completed before the Civil War was declared it was never delivered to the University of Mississippi, but to the Dearborn Observatory at Chicago.

In 1860 Barnard accepted an invitation of A. D. Bache to accompany a total eclipse expedition to Labrador. He returned to Newport, R. I., to find that he had been elected president of the American Association for the Advancement of Science. On account of the war the next meeting was not held until 1866, so that Barnard holds the record for length of office of president of the Association.

Barnard was a northerner, though a slaveholder, caught in the South at the outbreak of secession, of which he disapproved. Almost all the students entered military service and he resigned his office as Chancellor. The trustees of the University begged him to withdraw his resignation, which he did conditional upon the reopening of the University in the autumn. Such reopening did not take place so Barnard left with the good will of the trustees and a commission to report to them on military schools in South Carolina and Virginia. He made the report in person. President Jefferson Davis of the Confederacy personally urged him to stay in the South as he was needed to direct the work of obtaining sulphur from the mines of western Tennessee, but he declined and went with his wife to live at Norfolk, Virginia, until that city was captured by Federal troops in May, 1862.

Coming to Washington he was given direction of the map and chart department of the Coast Survey under A. D. Bache. This included the preparation and publication of war maps. While thus engaged he published his famous "Letter to the President

of the United States by a Refugee," which denounced slavery, the "giant conspiracy" of southern leaders and especially the work of northern Copperheads as the greatest danger faced by the Republic. Shortly after this letter appeared Barnard was elected tenth President of Columbia College, in 1864, at the age of fifty-five.

Barnard entered upon his work with energy and tact. He had to revive a feeble School of Mines. In his inaugural address, at a time when the conflict between science and religion was being much discussed, he took for his topic the real absence of such a conflict. As a priest and a man of science he sought to harmonize the two camps.

During the years of his presidency Barnard adhered closely to his duties of building the college into a university. He was appointed by the President of the United States on a government commission to the Paris Exposition of 1867 and to the exposition at Vienna, at both of which he was on a committee on instruments of precision. He made four other summer trips abroad, being everywhere received as a distinguished American citizen.

In the field of education he took a pleading part. As President Butler says: *

"Among the new visions which President Barnard had during his quarter century of service as administrative head of Columbia College were: the elective system of undergraduate study and the enriching of the undergraduate curriculum; the reform of the examination system; the emphasis which should be placed upon the study of modern European languages; the building of a university organization after the fashion of those of continental Europe upon the foundation of the undergraduate college; the provision of opportunities for the higher education of women, equal in all respects to those provided for men, this to be accomplished either through co-instruction of young men and young women in the same institution or by the establishment of separate colleges for women; the study of education as a science and the development of a plan for the professional training of teachers which should take its place side by side with plans already existing for the professional training of lawyers and physicians; and finally, the larger service of college and university to the general public which has since found expression in University Extension, in Home Study, and in various other forms of carrying the fruits of contemporary scholar-

* The Rise of a University. Vol. 1. Columbia Univ. Press. 1937.

ship to great companies of eager men and women who are no longer formal students at any institution."

As President of the College Barnard issued annual reports in which he set forth his views of education in general and the achievements and needs of the College. These writings have been republished in "The Rise of a University" (Vol. 1, 1937), by the Columbia University Press. They cover the whole field of education. As a man of science he early (1879) called for the need of provision of graduate instruction. It seems remarkable to us today that in 1882 the biological sciences were "all unrepresented in our scheme of instruction" at Columbia. He early urged that Columbia should follow Harvard in the adoption of the elective system, but he later came to see that undergraduates would be aided in choice of electives by the advice of a member of the faculty.

In May, 1888, Barnard presented to the Trustees of Columbia College his resignation as President. He was now in his eightieth year and his health unstable. These facts led to the acceptance of his request. He lived less than a year longer, dying April 27, 1889. In his will, being childless, he left a fund to the College for "encouraging scientific research." Also a fund for the increase of the library. He made provision for a gold medal to be awarded every five years to the person who shall "have made such discovery in physical or astronomical science or such novel application of science to purposes beneficial to the human race as, in the judgment of the National Academy of Sciences of the United States, shall be esteemed most worthy of such honor." Among recipients of this medal have been Niels Bohr, Sir William Henry Bragg, jointly with his son William L. Bragg, Albert Einstein, Warner Heisenberg, Edwin Hubble, Ernest Baron Rutherford.

Frederick Barnard, a scion of a family of professional men, lawyers, statesmen, physicians, military men, chemists, engineers and mathematicians, originally trained for the law, was led, on account of a family defect in hearing, into teaching and administration. He maintained chemical and astronomical research as an avocation.

Barnard was about six feet tall and in his later years grew a long white beard and reminded one of his former students of the conventional pictures of Moses. From youth he was gay in disposition and had an attractive personality. When in Hartford he arranged to go to a concert with some of Miss Beecher's girls when they should have attended one of her "exhibitions"; but the plot was discovered in time to foil it. From an early age he showed a somewhat non-plastic disposition. Thus when at school the tutor called him to account for some offence that Barnard did not regard as such, and demanded an apology, the boy refused and was publicly censured before the whole school. The Board of Trustees of the University of Alabama had voted unanimously a certain plan for instruction, Barnard wrote a full report opposing that plan and gained a partial victory. In later life at Columbia College he showed an intolerance of opposition and a certain imperiousness of manner. He had something of the warrior traits that were part of the family heritage.

Barnard wrote easily. He wrote many pieces of poetry, edited papers while at the University of Alabama and contributed to a literary journal. His exhaustive report on a proposition to modify the plan of instruction in the University of Alabama was written, while still busy with his usual college work, within six days. At his inauguration as Chancellor at Mississippi he wrote a long "Open Letter" to the Board of Trustees, urging scientific studies. His "Letter to the President of the United States" was an effective if somewhat exaggerated statement. His annual reports at Columbia were distinguished by fullness and clarity and had an immense influence. From 1873 to 1877 he was engaged in heavy literary work as Editor in Chief of Johnson's Cyclopedia containing 7,000 closely-printed pages; for which he wrote many important articles. In his last year he wrote much autobiographical material.

In speech he was equally a master of words. As a recent college graduate he made a Fourth of July speech. In 1851 at Alabama he again delivered a public oration; for "his eloquence was universally admired."

Barnard had variable moods. At twenty years while teaching, he would sit for a half hour at a time with his head bowed on

his desk, and his gloom was deepened by the incidence of deafness. At other times his gaiety was extreme.

Of him President Nicholas Murray Butler says:

"His activities were gravely limited and conditioned by his very severe deafness. In his office, he had standing on his desk a large wooden sound receiver, perhaps two feet square, into which one spoke when conversing with him. When himself speaking, his voice was naturally affected by his deafness and was neither clear nor pleasant. On the other hand, he had great charm of personality, manifested by his facial expression, by the character and cordiality of his speech and by his generous and kindly interest in those with whom he was associated.

"As a matter of fact, I owe to Dr. Barnard not only the choice of my career, but the determination to stick to it despite every sort and kind of temptation, whether financial or political."

Professor James C. Egbert of Columbia writes:

"I remember him very well indeed. He was a tall man with a long gray beard, very reverend in appearance, very dignified. I remember well how he conducted the commencement exercises, speaking most distinctly and appropriately. He was very deaf, and his desk was fitted up with a sort of amplifier through which the person who was calling upon him was compelled to speak. This was very awkward when one was particularly anxious to make an impression on him. I do not think that he was a man of ready temper; I should say that he was placid and not easily provoked. As I have suggested above, he had a most dignified bearing, especially in the presence of the students. Those who were interested in their work were always received readily by the President. I remember very well the letter which he wrote for me when I was thinking of seeking a position as a teacher. He was most kindly and fair in this statement, the sort of letter of recommendation which would have satisfactory effect. You will see that these statements which I have given are personal and indicate the impression which President Barnard made upon me. Many of us feel that President Barnard was most progressive in education and really had foresight as to the sort of institution Columbia should become and could become."

The following extract from the minutes of the Alumni of Columbia University give a contemporary estimate of President Barnard:

"In 1864 at the date of Doctor Barnard's accession to the presidency, the College was at a critical period of its history. It was ready for development and had begun to develop. The Law School had been established a few years previously and was in successful operation. The School of Mines was in process of organization. The Trustees had for several years

been considering the expansion of the undergraduate course, and in connection therewith a system of university education. At this critical period the College happily obtained as its chief counsellor and guide Dr. Barnard, a profound student of education, in sympathy with all the forms of higher development, literary as well as scientific, of quick perception, peculiarly open to new ideas and prolific of them, of learning deep, exact, and extensive in many fields, a classical and English scholar, a fine mathematician, physicist, chemist, and adding to his severer accomplishments that of being a poet and a musician of no mean quality, a prolific, elegant, and persuasive writer, a logical and convincing speaker, of sanguine enthusiastic temperament, bold and persistent in the advocacy of his opinions, and impervious to discouragement. He quickened into organic life the School of Mines, he gave vitalizing force to the extension and liberalization of the undergraduate course, to the founding of fellowships for the encouragement and assistance in their higher studies of earnest and able young men, to the extension of the library and the liberalization of its management, to the project of a course for the higher study of political and historical subjects, and to the scheme for a broad and liberal system of post-graduate or university instruction, which the College had long but vainly desired. In brief, he gave Columbia College a new life and a new significance, and by his commanding position in many learned societies, by the force and elegance of his published writings, scientific, literary, legal, political, educational, and by his wide acquaintance with the foremost men of his time, he attracted attention to the College, and did much to interest the community at large in it.

Age could not wither nor custom stale
His infinite variety.

He possessed, with such men as Gladstone and Bismarck (it is a very rare quality) the fervor in age that he had in youth, and was as ready as he was before he had secured position and fame, to take up a new idea, a new project, and pursue it with as much vigor as if a long life were still before him, and all his reputation yet to make. It was this quality that made him a great president to the very last. With almost his latest breath, unable to write, and speaking with difficulty, he dictated letters of counsel upon what was ever nearest his great heart—Columbia College and her future."

His scientific work was an avocation. On present day standards he might be regarded as a very gifted man whose other interests left him little time for fundamental work in science, although in photography his use of chlorine gas and invention of stereoscopic photography were real achievements. His knowledge and accuracy in using the sextant made him indispensable in locating the correct state boundary line. Again, in the Coast

Survey his work in preparing war maps required accuracy. He was in his day the foremost propagandist for the metric system in the United States. His analysis of the theory of magic squares reveals his hereditary mathematical genius. The examination of his bibliography, appended to this paper, gives the best idea of his scientific productiveness.

Barnard adapted himself well to any situation in which he found himself. While in the South there is no evidence that he entertained any strong feelings on slavery. Says Fulton, "He was not a man whose feelings governed his convictions." He accepted slavery as an unwelcome fact; and of his own will he became a slaveholder. After knowing him twenty years two southern gentlemen testified that they "had never heard his attachment to the institutions of the South called in question." He himself stated to his board, "I am a slaveholder and, if I know myself, I am sound on the slavery question." Yet after he had passed the war barrier and was safe in Washington, in the circle of abolitionist friends, he denounced "that relic of primeval barbarism, that loathsome monument to the brutalities of the ages of darkness, that monster injustice—cursed of Christian men and hated of God—domestic slavery."

The outstanding traits of Barnard, those that made him great, were the broad sweep of his imagination and his vision of the future, combined with the special gifts of administration and others that might have made him a noteworthy engineer. These, together with his capacity for making and holding friends and commanding support for his ideas, made him one of the great builders in the field of education.

SCIENTIFIC BIBLIOGRAPHY OF F. A. P. BARNARD

President Barnard's interests were wide. As clergyman, educationalist, pamphleteer and man of science, he wrote many magazine articles of a general nature and annual reports as President of Columbia University and gave many addresses (afterwards printed) which are not included in this bibliography. A complete bibliography has been compiled by Columbia University, New York City.

1830

A treatise on arithmetic. Hartford, Packard and Butler. 288 pp.

1831

Advertisement. In Bewick Bridge. A treatise on the construction, properties, and analogies of the three conic sections. New Haven, H. Howe.

1834

Education of the deaf and dumb. N. Amer. Rev., Apr., 38, pp. 307-357.

1836

Analytic grammar; with symbolic illustration. New York, E. French. 264 pp.

1838

On the Aurora Borealis of 14 Nov. 1837. Amer. Journ. Sci., 34, pp. 267-290.

1841

Improvement in the Daguerreotype process of photography. Amer. Journ. Sci., 41, pp. 352-354.

1844

Instruction of the deaf and dumb. N. Amer. Rev., Oct., 59, pp. 329-352.

1853

Theoretic determination of the expenditure of heat in the hot air engine. Amer. Journ. Sci., 2d ser. 16, pp. 218-227, 293, 351-357, 431-432.

Proposed modification of the construction of the Ericsson engine, with a view to increase its available power. Amer. Journ. Sci., 2d ser. 16, pp. 232-250.

Method of taking Daguerreotype pictures for the stereoscope, simultaneously, upon the same plate, with an ordinary camera. Amer. Journ. Sci., 16, pp. 348-350.

1854

On the elastic force of heated air, considered as a motive power. Amer. Journ. Sci., 2d ser. 17, pp. 153-168.

On the comparative expenditure of heat in different forms of the air-engine. Amer. Journ. Sci., 2d ser. 18, pp. 160-176.

Mechanical action of heat. Amer. Journ. Sci., 2d ser. 18, pp. 300-301.

1855

Improvement practicable in American colleges. A paper read before the American Association for the Advancement of Education, at its fifth annual session, in the chapel of the University of New York, on the 30th of August. Hartford, F. C. Brownell. 30 pp.

1856

On the theory which attributes the Zodiacal Light to a nebulous ring surrounding the earth. Amer. Journ. Sci., 2d ser. 21, pp. 217-237, 399-401.

1858

Report on the history and progress of the American coast survey, up to the year 1858. By the committee of twenty appointed by the American Association for the Advancement of Science, at the Montreal meeting, August, 1857. Cambridge, 1858. 126 pp.

1859

On the means of preventing the alteration of metallic surfaces employed to close and break a voltaic circuit. Amer. Assoc. Proc., pp. 208-215.

On the pendulum: with a description of an electric clock, constructed by E. S. Ritchie. Amer. Journ. Sci., 2d ser. 27, pp. 184-197.

1860

Notice of the astronomical expedition to Cape Chudleigh (or Chidley), Labrador. Amer. Journ. Sci., 2d ser. 30, pp. 281-285.

1862

Lectures on the undulatory theory of light. Annual Report Smithsonian Inst. pp. 107-239. Diagrs.

1863

Letter to the President of the United States, by a refugee. Philadelphia, J. B. Lippincott. 32 pp.

Hydraulics of the report on the Mississippi River of Capt. Humphreys and Lieut. Abbot. Amer. Journ. Sci., 2d ser. 36, pp. 16-37, 197-212.

On the explosive force of gunpowder. Amer. Journ. Sci., 2d ser. 36, pp. 241-256.

1865

Inaugural Discourse. In proceedings at the inauguration of Frederick A. P. Barnard, S.T.D., LL.D., as president of Columbia College, on Monday, October 3, 1864. New York. pp. 43-79.

1869

Machinery and processes of the industrial arts, and apparatus of the exact sciences. Report as U. S. Commissioner to the Paris Universal Exposition, 1867. Washington, Govt. Print. Off., 9, 146 pp. Diagrs.

1871

The metric system of weights and measures. Proc. of University Convocation. Albany, pp. 585-691.

A new form of binocular for use with high powers of the microscope. Read before the Microscopical Section of the American Association for the Advancement of Science, at the Troy meeting, 1871. Amer. Naturalist. Jan. vol. 4. 7 pp.

Improvement of American colleges. Columbia College: Elective studies. Amer. Journ. Education. 22, pp. 435-52.

The examination of Nobert's nineteenth band. Monthly Microscopical Journ. 6, pp. 194-198.

1872

Report on the principles which should govern the tolerance of deviations from standard weight of coins weighed in parcels; made to the Secretary of the Treasury of the U. S. at the request of the Commission of 1872 for the scrutiny of the coinage. Washington, Govt. Print. Off. 18 pp. Tables.

A few additional remarks on "The examination of Nobert's nineteenth band". Monthly Microscopical Journ. 7, pp. 119-122.

Money. *In* David Dudley Field, Outlines of an International Code. New York, pp. 281-296.

Weights and measures. *In* David Dudley Field, Outlines of an International Code. New York, pp. 297-349.

Longitude and time. *In* David Dudley Field, Outlines of an International Code. New York, pp. 350-361.

Sea Signals. *In* David Dudley Field, Outlines of an International Code. New York, pp. 362-366.

The metric system of weights and measures; an address delivered before the convocation of the University of the State of New York, Albany, Aug. 1, 1871. New York, Columbia College, 194 pp.

1874

International coinage. London, William Clowes and Sons. 8 pp.

Johnson's new universal cyclopaedia. Editors-in-chief, F. A. P. Barnard and Arnold Guyot. New York, A. J. Johnson, 3 vol.

1876

Theory of Magic Squares. Nat. Acad. Sci., April.

Magic Squares. New York, Johnson's Cyclopaedia, vol. iii.

1879

The possibility of an invariable standard of value. Proc. Amer. Metrological Soc. Dec.

The metric system of weights and measures; an address delivered before the convocation of the University of the State of New York, Albany,

Aug. 1, 1871. 3rd ed. with additions and index of two thousand references. Boston, Amer. Metric Bureau, 6, 456 pp. Tables.

Mono-metallism, bi-metallism and international coinage; a paper prepared for presentation to the Association for the Reform and Codification of the Law of Nations at their meeting held in the Guild Hall, London, Aug. 1879. New York, The S. W. Green Type-setting machines. 23 pp.

1880

The possibility of an invariable standard of value. Proc. Amer. Metrological Soc., vol. 2, pt. 1, pp. 65-84.

1881

Standard time. Proc. Amer. Metrological Soc. May, 14 pp.

The regulation of time; international coinage; the unification of weights and measures; sea-signals; read before the Association for the Reform and Codification of the Law of Nations, at Cologne, Aug. 1881. London, William Clowes and Sons. 16 pp.

The silver question and the international monetary conference of 1881. Princeton Rev. May, 57, pp. 342-368.

1882

Extracts from an address before the Association for the Reform and Codification of the Law of Nations, at Cologne, Aug. 1881. The regulation of time. Amer. Soc. Civil Eng. pp. 15-22.

The world's stock of the precious metals. New York, Gregory Bros. Proc. Amer. Metrological Soc., vol. 2, pt. 3, pp. 241-267.

1883

On the relation to the public welfare of changes in the volume of money and on monetary standards. Proc. Amer. Metrological Soc., vol. 2, pt. 3, pp. 202-230.

1884

The metrological system of the Great Pyramid. School of Mines Quarterly, 5, pp. 97-127, 193-217, 289-329.

The imaginary metrological system of the Great Pyramid of Gizeh. New York, J. Wiley & Sons. 106 pp.

The metrology of the Great Pyramid. Proc. Amer. Metrological Soc., vol. 4, pp. 117-219.

Pyramid metrology. The Churchman, Oct.

1888

Balance for determining specific gravities by inspection. Memoirs Natl. Acad. Sci. 4, pp. 203-206.

Theory of magic squares and of magic cubes. Memoirs Natl. Acad. Sci. 4, pp. 209-270.

Systems of weight and measure. School of Mines Quarterly, 9, pp. 193-212, 291-299.

Biography and Collected Addresses

John Fulton. 1896. Memoirs of Frederick A. P. Barnard, D.D., LL.D., L.H.D., D.C.L. Tenth President of Columbia College in the City of New York. New York, Macmillan. 485 pp.

William F. Russell. 1937. The Rise of a University. The Later Days of Old Columbia College. New York, Columbia University Press. 415 pp.

SOURCES OF INFORMATION

Besides the biographies and addresses, assistance in the compilation of this memoir was obtained from President Nicholas Murray Butler, Professor James C. Egbert, Dr. C. C. Williamson, Director of Libraries, and Mr. Milton Halsey Thomas, Curator of Columbiana, all of Columbia University, New York City.

Charles S. Hastings

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OF

CHARLES SHELDON HASTINGS

1848–1932

BY

HORACE S. UHLER

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

CHARLES SHELDON HASTINGS

1848–1932

BY HORACE S. UHLER

Charles Sheldon Hastings was born at Clinton, New York, on November 27, 1848.[1] His New England ancestry included an unusual proportion of professional men, particularly physicians. One of his great grandfathers, Dr. Seth Hastings, was born at Hatfield, Massachusetts, in 1745. After transferring his home to Washington, Connecticut, an eldest son was born to him in 1780 and given the name Seth, Jr. This grandfather of the subject of the present memoir subsequently moved to Clinton, New York, where he also practiced medicine and became, in the year 1816, the father of Panet Marshall Hastings. The latter graduated from Hamilton College at the age of twenty two, became a very prominent physician in Clinton, and gave lectures on anatomy and physiology at his alma mater. In the year 1843 Dr. P. M. Hastings married Jane Sheldon, a lady whose forebears were likewise sterling representatives of New England. About five years later this couple became the parents of Charles Sheldon, and when he was approximately six years old they changed to a permanent residence in Hartford, Connecticut.

In this city Hastings received his early training in the public schools and passed from the Hartford High School to the Sheffield Scientific School of Yale University in the fall of 1867. From this Institution he received the degree of Ph.B. in 1870 and then continued as a graduate student for a period of three more years. The diploma of Hastings' doctorate bears the date of the 26th of June 1873. During the last two years of his enrolment as a graduate student he held the position of Instructor in Physics in the Sheffield Scientific School. He then resigned in order to devote the next three years to study and travel in Europe. It was his inspiring privilege to attend

[1] Biographical notices of C. S. Hastings have been published by Frederick E. Beach and Frank Schlesinger. The respective references are: *Amer. Jour. Science*, **23**. 485 (1932) and *Astrophys. Jour.*, **76**, 149 (1932). With one exception, for which acknowledgment is made in the proper place, the present memoir owes nothing to these earlier papers.

courses given in Berlin by H. L. F. von Helmholtz and in Heidelberg by G. R. Kirchhoff. It may be interesting to record that Hastings' note-books and scattering memoranda indicate that he took Kirchhoff's lectures on optics which commenced on the 24th of April 1874, that he studied advanced mathematical analysis under Professor Königsberger, that he visited Steinheil's on March 11, 1875, and that in Paris during November 1875 he was buying scientific books as requisites to the courses at the Sorbonne. The sojourn in Paris was facilitated by the fact that Hastings was awarded the "Tyndall Scholarship" for the year 1875.

In the next year an event of paramount importance to the development of higher education in the United States of America and to the advancement of scientific research in the world occurred when the Johns Hopkins University was launched in Baltimore, Maryland. At the beginning there were appointed to this faculty—under the sagacious selecting by the first President of the University, Daniel Coit Gilman—six professors and seven "associates". In the list of associates, many of whom became leaders in their several departments of study, the name Charles Sheldon Hastings deservedly appears. Not one of these associates had attained the age of thirty years.

Hastings' academic title was first changed in the fall of 1882 to "Associate in Physics, Sub-Director of the Physical Laboratory, and Lecturer on Solar Physics." In 1883 the first three words of the title just quoted were replaced by "Associate Professor of Physics." He then resigned to accept the call to occupy the newly entitled [2] chair of Professor of Physics in the Sheffield Scientific School, New Haven, Conn.

Before passing to Hastings' career at Yale University the following fact merits presentation because it is presumably virtually unknown and it seems to be historically important. The fact is that the fields of applied optics, physics, and astronomy came very near losing, for a time at least but probably forever, the invaluable services of Hastings. In a notebook containing the long-hand manuscripts of several scientific

[2] Information relative to the founding of chairs in the Sheffield Scientific School may be obtained from the *National Cyclopaedia of American Biography*, vol. I, p. 172.

papers, most of which appeared later in print, there is to be found a letter simply indexed: "Letter to Scudder." [3] The full import of the matter may best be inferred from the letter itself which reads:

"Dec. 25, 1882.

"Your most flattering invitation to associate myself with you in editorial work has given me much more anxious thought than I had anticipated. At first I was strongly inclined to accept your offer, and had no doubt that the week granted me for consideration (for I supposed that an answer was not looked for before last Wednesday) would prove quite sufficient. At the end of that time, however, I was more uncertain than before, for this singular reason: I found that of the seven or eight friends whom I had consulted and whose judgment/opinions seemed to me of more weight than my own, all who were of nearly my own age advised me to go, whereas those who were considerably older strongly advised my remaining for the present in my position.

"The argument as urged by the latter was this: I am in a position which gives congenial work for which I have shown aptitude, and, although it is not such as ought to satisfy the ambition of a man of mature experience, it is one which yields its possessor valuable knowledge and secures him a constantly increasing number of friends. Such a change as contemplated means a change of work which, however kindly may be the judgments of my friends, I am by no means certain to prove well adapted, and from which an agreeable escape, in case the experiment were found to be a failure, would not be easy.

"The force of this reasoning must be granted, and I feel myself impelled to act upon it. In doing so my greatest regret is that I lose the opportunity of justifying in your own mind the gratifying things you have said to me and of me."

The following note written on the back of a list of physical apparatus then constituting the collection of the Sheffield Scientific School was doubtless very welcome and encouraging to the addressee.

"New Haven, Dec. 18, '83.

"My dear Hastings

"Here is the lean list of our apparatus. I am glad to know that $1000 has been put at your disposal by our Trustees to improve it. Mr. Richards, as you may know, has accepted, and it is a matter of course that both of you will be confirmed by the Corporation; so we are all well pleased. As ever

"Truly yours

C. S. Lyman"

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[3] Possibly Horace Elisha Scudder.

If Hastings needed a rest in the sense of a complete change of mental stimuli and physical environment the voyage to Caroline Island to observe a total eclipse of the sun came in most opportunely. The American Party, of which he was a responsible member, sailed from New York on March 2, 1883, crossed the Isthmus of Panama by train, and finally arrived at the coral island on Friday, April 20th. From Sunday March 11th until the date just given Hastings wrote a very interesting and instructive diary of his impressions and experiences, especially those which were received or occurred on the trip across the Isthmus and at the successive points of call: Buenaventura, Tumaco, Guayaquil, Pata, and Callao. The group of scientists arrived in San Francisco on June 11, 1883 after having been absent from the United States for one hundred and one days during which about 12,300 miles had been traveled, and fifty days had been spent aboard the U. S. S. *Hartford,* Admiral D. G. Farragut's flagship in the memorable battle of Mobile Bay.

The salient features of Hastings' progress having now been traced from the time of his birth until he became permanently settled in New Haven, Connecticut, as Professor of Physics in the Sheffield Scientific School, attention will be turned in succession to the three chief aspects or phases of his life. These may be conveniently designated as: his optical researches, his character as a teacher, and his fitness relative to his social environment.

It is difficult to state exactly when, or in what way, Hastings' interest in astronomy and optics was first strongly aroused. His early and lasting liking for botany, geology, and zoology was probably initiated and fostered by his companionable father who was also a natural philosopher in the original general sense of this term. Undoubtedly Hastings' ever increasing devotion to astronomy during his undergraduate years was largely due to the influence of Chester Smith Lyman whose academic chair in the Sheffield Scientific School included both physics and astronomy from 1871 to 1884. A single sheet of paper dated October 1, 1869 gives a brief account of the telescopic observations which Hastings had just been making, also one sketch of the rings of Saturn and another of the surface markings of Jupiter, and a diagram of the relative positions of four of the

satellites of the largest planet. This was recorded near the beginning of his senior year in college.

Less than ten years later his skill in making lenses and his keenness of observation were attracting attention outside of Baltimore. This is attested by the fact that the prominent astronomer, S. W. Burnham, wrote Hastings a short letter from Chicago, Illinois, on July 2, 1879, the closing paragraph of which reads: "I have heard something of your glass from Mr. Rockwell. I hope you will follow the thing up, and if it proves to be a success as I have no doubt it is, try it on a larger scale." (The objective referred to had an aperture of 4.1 inches.)

That Hastings did "try it on a larger scale" with extraordinary success is now an accepted fact of scientific history. Since Professor F. Schlesinger was Director of the Allegheny Observatory of the University of Pittsburgh from 1905 to 1920 he was in a position both geographically and by virtue of his special field to observe with interest, and to write authoritatively on, that which may be called for brevity the "Brashear-Hastings-McDowell Association." For these reasons, and with the freely-given permission of its author, the following quotation is made.[4]

"In the late eighties he [Hastings] received a letter from a correspondent in Pittsburgh, at that time unknown to him personally, which was to prove of the greatest importance in shaping his career. A few years before, John A. Brashear and his son-in-law, James B. McDowell, had started the ambitious project of establishing their optical factory, an undertaking that would have been altogether impossible in that day in this country had it not been for the moral and financial backing of William Thaw. After some difficult years this venture prospered and was soon standing upon its own feet. Its prosperity brought with it the necessity for a mathematical expert to take care of the demands that the growing sciences of astronomy and astrophysics were making upon the ingenuity of these opticians. Brashear put this problem to his friend Professor George F. Barker of the University of Pennsylvania, who suggested that he secure the co-operation of Hastings. Brashear wrote at once to this effect and Hastings accepted. This was just as it

[4] Frank Schlesinger. CHARLES SHELDON HASTINGS. *Astrophys. Jour.* **76**, 150-151 (1932). As supplementary reading reference should be made to JAMES B. McDOWELL—AN APPRECIATION, by J. S. Plaskett, Director of the Dominion Astrophysical Observatory. *Jour. Roy. Astron. Soc. Canada,* **18**, 185-193 (1924). The frontispiece shows both Hastings and McDowell in characteristic poses.

should have been. On the one hand, it gave Brashear and McDowell the technical advice without which they could hardly have developed as they did; and, on the other, it gave Hastings precisely the clinic he needed to put to use his then unrivaled skill and knowledge in matters optical.

"These three men remained associates until the death of Brashear in 1920 and that of McDowell in 1923. Their alliance produced among other large instruments the 72-inch reflector at Victoria, the 30-inch Allegheny photographic refractor, the 26-inch Yale photographic refractor at Johannesburg, the Swarthmore 24-inch visual refractor, and the Keeler reflector at Allegheny with all its complicated auxiliary apparatus. They have also provided observatories with many wide-field cameras, including the Bruce doublet for Barnard at the Yerkes Observatory and the twin 16-inch Bruce Camera for Max Wolf at Heidelberg. Almost all the many spectrographs that were installed in American observatories in the early years of this century owe at least something to Hastings' design, and some of them were built entirely by this firm. Spectrographs attached to visual refractors necessitate a correcting lens between the main objective and the slit, and these Hastings computed with great success. For the Allegheny refractor the writer put the converse problem to Hastings, namely, to design an auxiliary lens that would transform the color correction from that of a photographic telescope to a visual, without sensibly changing the position of the focal plane. This, I think, was the most strikingly successful achievement of Hastings and McDowell: they provided a 12-inch corrector which is interposed nearly halfway up the tube and which gives visual images that I defy any observer to distinguish from those obtained directly by a visual objective of the highest quality.

"Among the many other optical problems that engaged Hastings' attention may be mentioned the cause of the various types of solar and lunar halos, the design of an Aplanat magnifier (which has earned him the gratitude of scientific workers in many fields and in all quarters of the globe), better correction for color by the use of two special types of glasses or by three ordinary types, and the optical faults of the human eye."

Toward the end of his life, but definitely before his memory had practically vanished, Hastings was devoting all of his working hours and by far too much of his energy to the design of microscope lenses. At this time he designed and made a 10× ocular which (in his own words) "has an absolutely flat and rectilinear field. Theoretically it is superior in definition to the accustomed 10×, and some of our expert microscopists assert that it is so." On April 24, 1930 he finished making with his own hands and testing an incomparably fine objective of 16 mm focal length,

of numerical aperture 0.3, and consisting of only three discrete lenses. The memorandum written the next day reads: "The objective was finished yesterday and is now about as good as I can make it without beginning all over again. It still has a minute error of excentricity both in back and front. It requires ocular 25× to exhaust its powers and it will bear 30× very well. The most difficult object which I have succeeded in resolving (with dark field illumination) is Pl. Balticum (38000 lines per inch according to Van Heurck)."

Whenever Hastings achieved a material optical triumph he naturally exhibited the apparatus with justifiable pride to some of his friends and colleagues. In this instance his enthusiasm was so great that he assured me that his new lens system was at least one hundred per cent better than anything of its rating then on the market. In order to obtain an independent opinion on this matter I recently made a point of visiting a friend who has had much experience in studying microscopic objects. Although about eight years had elapsed since he made observations with Hastings' best microscope, he recollected the circumstances vividly and said that the exquisite details brought out by this optical system exceeded to such a degree anything which he had ever seen that it seemed as if a whole new world had been unveiled to his vision. Be this as it may, it should be stated, in behalf of unbiased scientific accuracy, that none of us made crucial quantitative tests of these lenses.

The slight residual imperfections in the lenses would assuredly have been entirely removed both theoretically and practically if Hastings could have found the kind of manual help and intelligent cooperation which he had become so accustomed to receiving from McDowell. He did make an appeal for experimental aid but apparently nothing was vouchsafed him. Hastings frankly confesses: "My skill in lens making is limited. Surfaces of short radii and plane surfaces I can manage very well, and, less satisfactorily, concave surfaces of long radius, but convex surfaces of long radius give me a lot of trouble to avoid zonal errors." The lenses in question have disappeared but the work-sheets which contain all of the data and calculations have been jealously preserved.

One thing about Hastings which has not been emphasized sufficiently is his general scholarship. He was really a scientific philosopher—a scholar of broad and accurate attainments. He was an excellent physicist, thoroughly versed in his specialty and fully conversant with the physics of his day. But he was more than that. He paid no little attention to the philosophical implications of science and to its cultural values. His asides during lectures on historical developments in physics and related subjects, the reasons for them and their significance, scientific and other, were always illuminating and consequently interesting and valuable. He nearly always taught more than just the topic he happened to be discussing at the moment. This also made the matter he was presenting stick more firmly in one's mind. Instead of being an isolated fact to remember, it was part of a connected whole which was manifestly incomplete without it. These characteristics were especially marked in his advanced course on optics which I attended as guest in the spring of the year 1911. In particular the influence of Helmholtz was quite apparent and it led the lecturer to say in substance that a thorough study of the eyes of vertebrates would constitute in itself an excellent course in optics.

Hastings was eminently successful as a teacher of undergraduates. His material was wisely chosen and carefully prepared in logical sequence, and the demonstration experiments always worked perfectly because of his unusual dexterity. An interesting sidelight on the reactions which Hastings aroused in undergraduate students in the first years of his teaching at the Johns Hopkins University is afforded by the following quotation from a little book written by Allen Kerr Bond, M.D., entitled *"When the Hopkins Came to Baltimore."* (The Pegasus Press, 1927.) Incidentally Doctor Bond was the second undergraduate to be examined for admission to the Collegiate Department.

"The instructor in Physics, Dr. Hastings, had a seraphic smile which appeared only when one of his pupils at the blackboard was heading for a fall. When we saw it break out over his face, we sure knew that Trouble was waiting for us around the corner. He was the only teacher I ever had who defamed Spelling. He said he had wasted endless hours learning spelling, which now-a-days he left entirely to the proof-reader, as beneath his own notice.

"He was very expert in lens-grinding and made the fine lens of the telescope in our Academy of Sciences on Franklin Street.

"It was at his Physics class that I first heard a phonograph. One day he brought in a machine which he said had been made for him in a little shop on Eutaw Street, where an expert mechanic was employed in making models of inventions such as Professor Rowland's spectrum gratings and things needed for demonstrations. He set the machine a-going and it related to us the tragic story of Jack and Jill,—ending with 'Jill came tumbling after *him.*' 'I know,' said Dr. Hastings, 'that the apprentice boy spoke that record; for that is the way he always recites it.' "

A few comments on the text-book known almost everywhere as "Hastings and Beach" should be made because at the time of its publication (1898) it undoubtedly set a standard above anything before it in this country. The book is difficult chiefly because of its vigor and the amount of ground which it covers with very brief discussions in general. Hence it was an excellent text for students with good heads and a bad one for those with poor heads—excellent for those who wanted to know physics as physicists and engineers but poor for those who desired to learn something about physics as part of a general education. A copy now before me contains an inscription which speaks eloquently for one class of head.

"If we should have another flood
"For safety hither fly.
"Although the earth would be submerged
"This book would still be dry."

Another book by Hastings—the mere existence of which is apparently known to only a few specialists—deserves consideration in this memoir because it is potentially the guarded answer of its author to the oft repeated suggestion of fear on the part of his intimate friends that his method of designing lenses might be lost to posterity because most, if not all, of his research papers in this field give results and not specific directions. The title is *NEW METHODS IN GEOMETRICAL OPTICS with Special Reference to the Design of Centered Optical Systems.* (The Macmillan Co., 1927.) The opinions expressed below concerning the character of this volume should be nearly correct because they are based on actual experience with its contents in two different ways. In the first, a general

but rather superficial view was obtained by reading the page proof. In the second, I used the book as a text in the next giving of the first part of my graduate course on geometrical optics and the theory of spectroscopic apparatus. Although the book was not particularly designed for class use, I was surprised and disappointed to find that the pedagogical experiment did not succeed. The chief reason seemed to be that the material presented had been extremely condensed. After finding that it usually required from six to eight hours of computation and preparation on my part to discover precisely how Hastings had obtained even one of his illustrative numerical tables the conviction became gradually forced upon me that, aside from the easy purely mathematical analysis,[5] the text was essentially a compilation of results which its author had accumulated during his life-long experience as consultant and theorist for the John A. Brashear Optical Co. Nevertheless it should not be inferred from the preceding critique that this text is useless, rather it is "necessary but not sufficient", to borrow a mathematical phrase which fits the case admirably. The Rosetta stone is to be found in Hastings' work-sheets which give implicitly the trains of thought followed while repeating the calculations (with only a single card of four-place logarithms) until the errors of the centered optical system under design were made either to vanish or at least to satisfy the required theoretical tolerance. In other words, if Hastings had supplemented his text by including a long appendix exhibiting *all* of the tedious arithmetical labor which he patiently performed in the case of any one compound lens, then a properly prepared reader could find out precisely how to copy, continue, and perhaps extend the master's theoretical designing. Obviously this would not remove the necessity for a great deal of concurrent laboratory work.

A careful study of the available records showed that no graduate student has ever attempted to obtain one of the higher academic degrees by doing his thesis work under Hastings' guidance and in the special field of the latter. This fact may be

[5] Hastings uses the marks $\underline{\ \ }\!\rfloor$ and ! in his original paper (1893) and in this book, respectively, to denote a continued product and not a factorial. This may lead to confusion because the accepted mathematical symbol is capital pi.

explained on the basis of the following deterrent influences: the drudgery and consumption of time inherent in the computations referred to above, lack of experimental facilities and especially optical glass, and above all the unsympathetic attitude which Hastings invariably took whenever he was asked a direct question about the practical application of the mathematical theory which he gave in his advanced course in optics. This selective taciturnity may have been accidental or temperamental but since, to his intimate friends at least, he was without exception a very approachable person it seems far more probable that the trait was constructively developed for defensive purposes.

Before considering Hastings' personality and social relations a few "fragments" gathered from his loose sheets of paper and note-books will be recorded because some of them may be of interest and probably not one occurs elsewhere in print. Considerations of brevity and of continuity of thought precluded the possibility of incorporating them in the preceding pages.

There exists the original white-on-black drawing by Hastings of satellites I, III, and IV "of Jupiter as seen Sept. 1, 1869." This is accompanied by a printed proof which implies publication. All endeavors at finding the reference have failed. The date corresponds to the beginning of his senior year.

Hastings early discovered an object in the constellation of Taurus to which he refers as "my double star". His first extant memorandum is dated Friday, Oct. 1, 1869. "New double in Taurus divided or rather disks just in contact with 450 solid." [Meaning solid ocular and magnification 450×.] "Compared it to γ^2 Androm. and it seemed a little closer and more difficult on account of faintness." "The components are of nearly equal size." On Feb. 2, 1882 Asaph Hall wrote a letter (from the U. S. Naval Observatory, Washington, D. C.) to Hastings in which he said: "Your star is a fine double. Last night I found $p=298^{\circ}.3$: $s=0''.54$ the night was only middling; and on a good night it would be an easy object here. When and with what glass did you find it?" On Feb. 11, 1882 A. Hall gave the magnitudes of the components as 8½ and 8.

Oct. 10, 1880. "Compared Steinheil's triplets very carefully

with my solid eyepieces of nearly equivalent powers. I could find no inferiority in point of definition or light, though the available field of mine is about 20% greater." Oct. 20, 1880. "New ¾ in. and $\frac{3}{10}$ in. solid eyepieces; they are very good."

"The glass circle spectrometer seems to have been completed April 22, 1886."[6]

March 2, 1889. "Finished this day the 2¾ in. objective . . ." "It performs beautifully on all kinds of objects . . ." "The following observations were made to test the power of a telescope of 2¾ in. aperture and 38 in. focal length with perfect color correction. The comparisons are made with a 2⅝ in. telescope of 33 in. focal length (the first telescope that I made) which is constructed on the Herschelian type and very admirably corrected." This first telescope antedates Oct. 8, 1880 if it is the same one referred to on that date in the following quotations. "Observed with 2⅝ in. telescope." "Keeler's telescope with 2½ in. objective has slight negative spher. aber. and marked deficiency of light in comparison with mine."

June, 1915. "Finished new type of solid solar ocular (i. e., solid with cemented slip of dark glass inside, to be used with Herschel prism.) Highly satisfactory."

During the years 1920, '21, and '22 Hastings acted as optical adviser for the Prisma Company which was experimenting on the production of colored motion pictures. In this connection it may be worthy of note that the friendship which it was my great privilege to share with Hastings grew out of the very lively interest that we both took in the monochrome motion pictures at the time when the art of pantomime and suggested repartee was at the peak of its development.

In a certain letter dated April 23, 1930: "I am about to send you for inspection my 10× ocular and one of my 16 mm 0.25 objectives." "Please note besides its defining power (with high power ocular) its great working distance and the fact that, the front being removed, the back forms an excellent objective of 32 mm 0.12. The last feature is one which, I should imagine, would be of commercial value. My most

[6] See F. E. Beach, *loc. cit.*, 486.

valuable invention, however, if I except my military telescope, is my 16 mm 0.3 objective together with what it promises. It is of three lenses only . . ."

Honors were received by Hastings from many quarters. At the ninth Cincinnati Industrial Exposition he was awarded, on October 8, 1881, a silver medal for his "Telescope Object Glass." On November 19, 1884 he was elected a member of the Connecticut Academy of Arts and Sciences. To this he tendered his resignation in the year 1915. On April 18, 1889 he was elected to membership in the National Academy of Sciences. In the same year he was appointed an *officier de l'instruction publique* in France. As a member of the committee on photographic proofs and apparatus, for the General Paris Exposition of 1889, he received a commemorative diploma on September 29th. At the Paris Exposition of 1900 he was awarded a gold medal on August 18th. He was elected a member of the American Philosophical Society at a meeting held in Philadelphia, Penna., on April 18, 1906. In 1926 the Franklin Institute of Philadelphia awarded Hastings a medal for the improvements he had made in optical instruments. He was a fellow of the American Association for the Advancement of Science, also of the American Physical Society, an honorary member of the Societá degli Spectroscopisti Italiani, and a collaborating editor of the *Astrophysical Journal*. The Physical Club of Yale University was founded in the autumn of 1899, and when, on October 31st, the first meeting was held the management of the club was placed in the hands of an executive committee consisting of Professors J. W. Gibbs, C. S. Hastings, and A. W. Wright.

Hastings' happy disposition and magnetic personality won him many friends not only among scientists but also among cultured people in general. In New Haven he belonged to two distinct sets. The members of one of these represented the University circle and were engaged in intellectual pursuits. The other set was composed in the main of men prominent in banking, law, manufacturing, etc. He was an active member of the select Colby Club, a group which met on alternate Saturday evenings when a member read a half-hour paper on some cultural sub-

ject. For example, a paper by Hastings bore the title: "On Certain Limitations in Science." He was a charter member of the New Haven Lawn Club Association, and president of the exclusive Graduates Club for the three years beginning with 1905.

The ruddy complexion and vigorous health of Hastings were due to his taking plenty of outdoor exercise. He was especially fond of bicycling, often with his daughter, both in this country and in England. He played tennis until fairly late in life, and even after this he continued swimming in season at his summer home in the town of East River, Connecticut. He derived much pleasure and recreation from fishing, particularly on the yachting excursions to southern waters which were made every spring and fall at the invitation of a certain wealthy friend.

In 1878 Hastings married Elizabeth Tracy Smith of Hartford. About three years later their only child was born and baptized as Katherine Panet. The daughter became Mrs. Horace W. Chittenden. She presented her father with four grandchildren, three girls and finally a boy. After a protracted illness Mrs. Hastings died in the fall of 1930. Although Hastings was not a finished musician he did enjoy playing the flute to the piano accompaniment by his wife.

An interesting sidelight is thrown on Hastings' consistent equanimity and contagious cheerfulness by something that was brought out in the course of a discussion on the philosophy of "happiness". His creed was that happiness is a quality which has to be "learned" by one's own efforts. In reply to a recent inquiry of mine Mrs. Chittenden wrote: "He certainly learned it for himself, and even in his last long illness, when so little was left to him, he was almost entirely cheerful and found contentment and happiness in little simple things." Hastings was not a member of any church in New Haven but he often attended services at St. Johns Episcopal Church with his wife and daughter. It is conjectured that he joined a Congregational church when as a youth he dwelt in Hartford, Connecticut. That he pondered over spiritual problems is established by the fact that on several occasions he propounded to me in all seriousness abstruse questions concerning the concept of the

Holy Ghost. On Sunday, January 31, 1932 Hastings died at his daughter's home in Greenwich, Connecticut. On the afternoon of the following Wednesday a small group of us motored to Cedar Hill Cemetery, Hartford, to attend the last services held over the actual ashes of an irreplaceable friend.

BIBLIOGRAPHY OF CHARLES S. HASTINGS

1873

Comparison of the Spectra of the Limb and the Center of the Sun; *American Journal of Science and Arts,* **5**, 369-371. Reprinted in *Les Mondes,* **32**, 702-704; *Nature,* **8**, 77.

1878

On the Influence of Temperature on the Optical Constants of Glass; *American Journal of Science and Arts,* **15**, 269-275.

1879

On Triple Objectives with Complete Color Correction; *ibid.,* **18**, 429-435.

On Lockyer's "Hypothesis That the So-called Elements Are Compound Bodies"; *American Chemical Journal,* **1**, 15-26.

1880

Report on the Total Solar Eclipse of July 29, 1878; *Washington Observations for 1876,* Appendix, III, 153-162.

A Synopsis of the Scientific Writings of Sir William Herschel [with Edward S. Holden]; *Ann. Rep. of the Smithsonian Institution,* 509-622.

1881

A Theory of the Constitution of the Sun . . .; *Proceedings of the American Academy of Arts and Sciences,* **16**, 140-152. Also in *Spectroscopic Italian Memoirs,* **9**, 204; *American Journal of Science,* **21**, 33-44; *Philosophical Magazine,* **11**, 91-103.

1882

On the Color Correction of Double Objectives; *American Journal of Science,* **23**, 167-176.

Comet Cruls; *Sidereal Messenger,* **1**, 171-174.

On Modifications of the Spectrum of Sodium Vapor in a Bunsen Flame [abstract]; *Proceedings of the American Association for the Advancement of Science,* 218-220.

A Note on Triple Objectives; *Johns Hopkins University Circulars.* No. 15, May 13th, 216.

On Some Recent Experiments in the Construction of Telescope Objectives; *ibid.,* No. 19, **2**, 9-10.

1883

List of Twenty-three New Double Stars, Discovered at Caroline Island, . . .; *Science,* **2**, 66.

Observations of 23 New Double Stars; *Memoirs of the National Academy of Sciences,* **2**, 97-98.

Report on the Eclipse Expedition to Caroline Island, May 1883; *ibid.*, **2**, 102-125.

The Phenomena Presented by the Atmosphere of Venus During a Transit; *Sidereal Messenger*, **1**, 273-278.

Recent Experiments in the Construction of Telescope Objectives; *ibid.*, **1**, 244-246; **2**, 39-44.

On the Variation in the Focal Length of an Objective with Change of Temperature; *Astronomische Nachrichten*, **105**, 69-72.

1884

A Singular Optical Phenomenon; *Science*, **3**, 501-502.

Inertia; *ibid.*, **3**, 559-560.

1888

On the Law of Double Refraction in Iceland Spar; *American Journal of Science*, **35**, 60-73.

1889

A General Method for Determining the Secondary Chromatic Aberration for a Double Telescope Objective . . .; *ibid.*, **37**, 291-298.

Physics of Chemistry; *An Elementary Text-Book of Chemistry* by William G. Mixter, 1-45.

Spectral Analysis; *ibid.*, 90-94.

1891

The Ladd Observatory [an address]; Providence, R. I., 24-26.

The History of the Telescope; *Sidereal Messenger*, **10**, 335-354. Also in *Annual Report of the Smithsonian Institution*, 95-109.

1893

On Certain New Methods and Results in Optics; *Memoirs of the National Academy of Sciences*, **6**, 37-47.

1894

Report on Specimens of Crown and Flint Glass Belonging to the Lick Observatory: Part II; *Publications of the Lick Observatory*, **3**, 133-152.

1896

Armand Hippolyte Louis Fizeau; *Astrophysical Journal*, **4**, 367-368.

1898

A Text-Book of General Physics [in collaboration with Frederick E. Beach]; Ginn and Co. viii + 768.

1899

On a New Type of Telescope Objective Especially Adapted for Spectroscopic Use; *American Journal of Science,* 7, 267-270; *Astrophysical Journal,* **9**, 162-166.

1900

James Edward Keeler; *American Journal of Science,* **10**, 325-326.

1901

Light; Chas. Scribner's Sons. xi + 224.

1903

Biographical Memoir of James Edward Keeler; *Biographical Memoirs of the National Academy of Sciences,* **5**, 231-246.

1905

On the Optical Constants of the Human Eye for Different Colors; *American Journal of Science,* **19**, 205-209.

The Error of Collimation in the Human Eye; *ibid.,* 310-314.

On a Group of Visual Phenomena Depending Upon Optical Errors of the Human Eye; *ibid.,* 401-412.

On Errors of Eccentricity in the Human Eye; *American Journal of Physiology,* **13**, 304-308.

1909

Simon Newcomb; *American Journal of Science,* **28**, 290-292.

Josiah Willard Gibbs; *National Academy of Sciences, Biographical Memoirs,* **6**, 375-393.

Architectural Refinements in Medieval Churches, Computed; *Architectural Review,* **26**, 132-139.

1914

On Halos [extract from "Light"]; *Monthly Weather Review,* **42**, 617-619.

1915

Halo of May 20, 1915, at New Haven, Conn.; *ibid.,* **43**, 215-216.

Halo of May 20, 1915, Analyzed; *ibid.,* 498-499.

1916

Arthur Williams Wright; *American Journal of Science,* **41**, 361-366.

1920

A General Theory of Halos; *Monthly Weather Review,* **48**, 322-332.

1924

The Shadow Bands of Total Eclipses; *Popular Astronomy,* **32**, 411-412.

1925

A New Type of Telescope Especially Adapted to Spectrometers; *Journal of the Optical Society of America,* **11**, 63-65.

1926

Astronomical Oculars; *Popular Astronomy,* **34**, 375-379. Reprinted in *Amateur Telescope Making,* Scientific American Publishing Co., 144-147 (1933).

1927

New Methods in Geometrical Optics; Macmillan Co., vii + 103.

1928

The Foucault Pendulum of the Peabody Museum of Yale University; *American Journal of Science,* **16**, 147-152.

Wallace H Carothers

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OF

WALLACE HUME CAROTHERS

1896–1937

BY

ROGER ADAMS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1939

WALLACE HUME CAROTHERS

1896–1937

BY ROGER ADAMS

Wallace Hume Carothers, who died on April 29, 1937, was born in Burlington, Iowa, on April 27, 1896. His contributions to organic chemistry were recognized as outstanding and, in spite of the relatively short span of time for his productive accomplishments, he became a leader in his field with an enviable international reputation.

His paternal forbears were of Scotch origin and settled in Pennsylvania in prerevolutionary days. They were farmers and artisans. His father, Ira Hume Carothers, who was born in 1869 on a farm in Illinois, taught country school at the age of 19. Later he entered the field of commercial education and for forty-five years has been engaged in that type of work as teacher and vice-president in the Capital City Commercial College, Des Moines, Iowa. Wallace Hume Carothers was the first scientist in the family.

His maternal ancestors were of Scotch-Irish stock and were also, for the most part, farmers and artisans. They were great lovers of music, and this may account for the intense interest in and appreciation of music which Carothers possessed. His mother, who was Mary Evalina McMullin of Burlington, Iowa, exerted a powerful influence and guidance in the earlier years of his life.

To his sister Isobel (Mrs. Isobel Carothers Berolzheimer), of radio fame as Lu in the trio Clara, Lu and Em, he was especially devoted. Her death in January, 1936, was a staggering shock to him and he was never able to reconcile himself completely to her loss.

On February 21, 1936, he married Helen Everett Sweetman of Wilmington, Delaware. Her father is Willard Sweetman, an accountant, and her mother, Bertha Everett. The family is of English-Welsh descent. Mrs. Carothers received her bachelor's degree in chemistry at the University of Delaware in 1933 and was employed in the patent division of the chemical depart-

ment of the du Pont Company from 1933-1936. A daughter, Jane, was born November 27, 1937.

Wallace was the oldest of four children. His education began in the public schools of Des Moines, Iowa, to which city his parents moved when he was five years of age. In 1914 he graduated from the North High School. As a growing boy he had zest for work as well as play. He enjoyed tools and mechanical things and spent much time in experimenting. His school work was characterized by thoroughness and his high school classmates testify that when he was called upon to recite his answers revealed careful preparation. It was his habit to leave no task unfinished or done in a careless manner. To begin a task was to complete it.

He entered the Capital City Commercial College in the fall of 1914 and graduated in the accountancy and secretarial curriculum in July, 1915, taking considerably less time than the average. He entered Tarkio College, Tarkio, Missouri, in September, 1915, to pursue a scientific course, and simultaneously accepted a position as assistant in the Commercial Department. He continued in this capacity for two years and then was made an assistant in English, although he had specialized in chemistry from the time he entered college. During the World War the head of the department of chemistry, Dr. Arthur M. Pardee, was called to another institution, and Tarkio College found it impossible to secure a fully equipped teacher of chemistry. Carothers, who previously had taken all of the chemistry courses offered, was appointed to take over the instruction. Since he was rejected as a soldier on account of a slight physical defect, he was free to serve in this capacity during his junior and senior years. It is interesting that during his senior year there were four senior chemistry-major students in his class and every one of them later completed work for the doctorate, studying in the universities of this country and abroad. Today they bear testimony to the fact that as undergraduates they owed much to the inspiration and leadership of Carothers.

Upon entering college his interest in chemistry and physical sciences was immediate and lasting, and he rapidly outdistanced his classmates in accomplishment. As a student he showed

mature judgment and was always regarded by his fellow students as an exceptional person. Invariably he was the brightest student in the class regardless of the subject. Financial necessity required that he earn a large portion of his educational expenses. He always found time, however, to associate with the other students, though he showed little interest for the boisterous enthusiasms of the average underclassman. During his last two years in college he was entrusted with a number of student offices to which he gave freely of his time and energy.

Leaving Tarkio College in 1920 with his bachelor of science degree, he enrolled in the chemistry department of the University of Illinois where he completed the requirements for the master of arts degree in the summer of 1921. His former instructor at Tarkio College, then head of the chemistry department at the University of South Dakota, desired a young instructor to handle courses in analytical and physical chemistry and was fortunate in securing Carothers for this position during the school year, 1921-1922. He went to South Dakota only with the intention of securing sufficient funds to enable him to complete his graduate work, but the careful and adequate preparation of his courses, as well as his care of the students under his direction, showed that he could be a very successful teacher of chemistry. He was still the same quiet, methodical worker and scholar, not forceful as a lecturer, but careful and systematic in his contact with the students. He always required adequate preparation of assigned work and was able to get a large volume in student accomplishment.

Simultaneously with his teaching work he started to develop some independent research problems. He was especially interested in the 1916 paper of Irving Langmuir on valence electrons and desired to investigate some of the implications it held in organic chemistry. Pursuing this idea he carried out laboratory studies which were reported in his first independent contribution to the Journal of the American Chemical Society, "The Isosterism of Phenyl Isocyanate and Diazobenzene-Imide." His second independent paper, published while still a student, was that on "The Double Bond." In this he presented the first clear, definite application of the electronic theory to organic

chemistry on a workable basis. He described the electronic characteristics of the double bond and in essence included in his discussion everything that has since been written on this particular subject.

It was evident, even at this stage of his career, that teaching was not his forte. Literally he spent all of his spare time on research problems in which he became interested. A number of his newly found friends in South Dakota tried to induce him to relax somewhat from his constant and sustained application to work, but without avail. He appeared to be driven by the many things that occurred to him as worthy of investigation in the laboratory.

He returned to the University of Illinois in 1922 to complete his studies for the degree of doctor of philosophy, which he received in 1924. His major work was in organic chemistry with a thesis under the direction of Dr. Roger Adams, on the catalytic reduction of aldehydes with platinum-oxide platinum-black and on the effect of promoters and poisons on this catalyst in the reduction of various organic compounds. His minors were physical chemistry and mathematics. He exhibited the same brilliance in all of his courses and in research which characterized his earlier accomplishments. Although specializing in organic chemistry, he was considered by the physical chemists to have a more comprehensive knowledge of physical chemistry than any of the students majoring in that field. In 1920-1921 he held an assistantship for one semester in inorganic chemistry and for one semester in organic chemistry. He was a research assistant during 1922-1923, and during 1923-1924 held the Carr Fellowship, the highest award offered at that time by the department of chemistry at Illinois. During these two years his seminar reports demonstrated his wide grasp of chemical subjects. The frequency with which his student colleagues sought his advice and help was indicative of his outstanding ability. At graduation he was considered by the staff as one of the most brilliant students who had ever been awarded the doctor's degree. A vacancy on the staff of the chemistry department of the University of Illinois made it possible to appoint him as an instructor in organic chemistry in the fall of 1924. In this

capacity he continued with unusual success for two years, teaching qualitative organic analysis and two organic laboratory courses, one for premedical students and the other for chemists.

Harvard University, in 1926, was in need of an instructor in organic chemistry. After carefully surveying the available candidates from the various universities of the country, Carothers was selected. In this new position he taught during the first year a course in experimental organic chemistry and an advanced course in structural chemistry, and during the second year he gave the lectures and laboratory instruction in elementary organic chemistry.

President James B. Conant, of Harvard University, was professor of organic chemistry at the time that Carothers was instructor. He says of him—

"Dr. Carothers' stay at Harvard was all too short. In the brief space of time during which he was a member of the chemistry department, he greatly impressed both his colleagues and the students. He presented elementary organic chemistry to a large class with distinction. Although he was always loath to speak in public even at scientific meetings, his diffidence seemed to disappear in the classroom. His lectures were well ordered, interesting, and enthusiastically received by a body of students only few of whom planned to make chemistry a career. In his research, Dr. Carothers showed even at this time that high degree of originality which marked his later work. He was never content to follow the beaten track or to accept the usual interpretations of organic reactions. His first thinking about polymerization and the structure of substances of high molecular weight began while he was at Harvard. His resignation from the faculty to accept an important position in the research laboratory of the du Pont Company, was Harvard's loss but chemistry's gain. Under the new conditions at Wilmington, he had facilities for carrying on his research on a scale that would be difficult or impossible to duplicate in most university laboratories. Those of us in academic life, however, always cherished the hope that some day he would return to university work. In his death, academic chemistry, quite as much as industrial chemistry, has suffered a severe loss."

In 1928 the du Pont Company had completed plans to embark on a new program of fundamental research at their central laboratory, the Experimental Station at Wilmington, Delaware. Carothers was selected to head the research in organic chemistry. The decision to leave his academic position was a difficult one. The new place demanded only research and offered

the opportunity of trained research men as assistants. This overbalanced the freedom of university life and he accepted. From then on until his death his accomplishments were numerous and significant. He had the rare quality of recognizing the significant points in each problem he undertook, and unusual ability for presenting his results in a most explicit and precise way, which led to clarity and understanding. In these nine years he made several major contributions to the theory of organic chemistry and discoveries which led to materials of significant commercial importance. Dr. Elmer K. Bolton, Chemical Director of the du Pont Company, writes concerning Carothers—

"At the time the du Pont Company embarked upon its program of fundamental research in organic chemistry in the Chemical Department, Dr. Carothers was selected to direct this activity, because he had received the highest recommendations from Harvard University and the University of Illinois, and was considered to have unusual potentiality for future development. There was placed under his direction a small group of excellently trained chemists to work on problems of his own selection. The results of his work, extending over a period of nine years, have been of outstanding scientific interest and have been considered of great value to the Company as they have laid the foundation for several basically new developments of commercial importance.

"In our association with Dr. Carothers, we were always impressed by the breadth and depth of his knowledge. He not only provided inspiration and guidance to men under his immediate direction, but gave freely of his knowledge to the chemists of the department engaged in applied research. In addition, he was a brilliant experimentalist. Regarding his personal characteristics, he was modest, unassuming to a fault, most uncomplaining, a tireless worker—deeply absorbed in his work, and was greatly respected by his associates. He suffered, however, from a nervous condition which in his later years was reflected in poor health and which became progressively worse in spite of the best medical advice and care, and the untiring efforts of his friends and associates. His death has been a great loss to chemistry and particularly to the Chemical Department. In my judgment, he was one of the most brilliant organic chemists ever employed by the du Pont Company."

His reputation spread rapidly; his advice was sought continually, not only by his colleagues but also by chemists throughout the world. In 1929 he was elected Associate Editor of the Journal of the American Chemical Society; in 1930 he became

an editor of Organic Syntheses. He took an active part in the meetings of the organic division of the American Chemical Society. He was invited frequently to speak before various chemical groups. He addressed the Johns Hopkins summer colloquium in 1935 on "Polymers and the Theory of Polymerization." That year he also spoke on the same subject before the Faraday Society in London, when his paper was considered one of the outstanding presentations on the program. His achievements were recognized by his election to the National Academy of Sciences in 1936—the first organic chemist associated with industry to be elected to that organization. During these years from 1928-1937 several attractive academic positions were offered him but he chose to remain to the end with the company which had given him his opportunity for accomplishment.

Very early in life he displayed a love for books. From the time when Gulliver's Travels interest a boy on through Mark Twain's books, Life of Edison, and on up to the masters of English literature, he was a great reader. He possessed a singing voice that might have developed under training into something very worthwhile. Though he had no technical training in music, he was a lover of the great masters, and possessed a large and much-used collection of phonograph records of their works. He said occasionally that were he to start over he would devote his life to music.

Carothers was deeply emotional, generous and modest. He had a lovable personality. Although generally silent in a group of people, he was a brilliant conversationalist when with a single individual, and quickly displayed his broad education, his wide fund of information on all problems of current life, and his critical analysis of politics, labor problems and business, as well as of music, art, and philosophy. With all his fine physique he had an extremely sensitive nature and suffered from periods of depression which grew more pronounced as he grew older, despite the best efforts of his friends and medical advisors.

SCIENTIFIC WORK

His early scientific work involved an extension to organic compounds of Langmuir's idea of isosterism. He demonstrated

that it was valid in the case of phenyl isocyanate and azoimide. Reactions of the double bond were interpreted in terms of the electronic theory, using a point of view that has since gained wide acceptance.

His next efforts were devoted to demonstrating that any idea of "negativity" alone is inherently incapable of accounting for the relative reactivity of organic halides. He measured the base strength of a series of amines. His work on the thermal decomposition of alkali alkyls threw light on the inherent properties of the simplest organic anions.

The first field of which he was in a position to make an exhaustive study was that of acetylene polymers and their derivatives. With vinylacetylene and divinylacetylene made available to him, he completed a detailed study of these substances. It was his discovery that it was possible to add hydrogen chloride to monovinylacetylene with formation of 2-chloro-1, 3-butadiene, called chloroprene. This substance is analogous structurally to isoprene but polymerizes several hundreds of times more rapidly and leads to a product much superior to all previously known synthetic rubbers. It was the first synthetic material to show rubber's curious property of developing fibrous orientation when stretched and instantly reverting to the amorphous condition when released from stress. In resistance to aliphatic hydrocarbons and to most chemical reagents it is definitely superior to natural rubber. It has, moreover, a greater resistance than rubber to corona and sunlight. Carothers' work laid the foundation for the development by other chemists and by chemical engineers of the du Pont Company of the commercial product which has found wide industrial use and which is marketed as neoprene.

These practical results, however, were of no greater importance than the theoretical. In the course of the investigation, many analogs and homologs of chloroprene were prepared and studied. Their behavior threw light on the relationship between the chemical structure of a diene and its suitability as a precursor of rubber. Fundamental information concerning the character and formation of the various polymers from these compounds was revealed and their structures clarified. The reactivity of

the vinylacetylenes and the mechanism by which the products formed was studied in detail. New light was thrown on 1,4 addition and on α,γ rearrangements. His work in this field was a basic contribution to acetylene chemistry.

The most outstanding scientific accomplishment of Carothers was his work on linear polymers. In a letter written to Dr. John R. Johnson of Cornell University on February 14, 1928, Carothers made a statement which demonstrated the careful thought and study which he had given previously to polymerization and polymeric molecules. It follows—

> "One of the problems which I am going to start work on has to do with substances of high molecular weight. I want to attack this problem from the synthetic side. One part would be to synthesize compounds of high molecular weight and known constitution. It would seem quite possible to beat Fischer's record of 4200. It would be a satisfaction to do this, and facilities will soon be available here for studying such substances with the newest and most powerful tools.
>
> "Another phase of the problem will be to study the action of substances xAx on yBy where A and B are divalent radicals and x and y are functional groups capable of reacting with each other. Where A and B are quite short, such reactions lead to simple rings of which many have been synthesized by this method. Where they are long, formation of small rings is not possible. Hence reaction must result either in large rings or endless chains. It may be possible to find out which reaction occurs. In any event the reactions will lead to the formation of substances of high molecular weight and containing known linkages. For starting materials will be needed as many dibasic fatty acids as can be got, glycols, diamines, etc. If you know of any new sources of compounds of these types I should be glad to hear about them."

These initial ideas culminated in the publication of a series of thirty-one papers in the field of polymerization. In these he proposed a general theory of condensation-polymerization and a logical and systematic terminology suitable for use in this previously disorganized field. The implications of his theory were illustrated by a series of experimental studies dealing with polyesters, hydrocarbons, polyamides, and polyanhydrides. These studies provided experimental material for correlating chemical structure and physical properties of materials of high molecular weight, and furnished evidence favoring a view now generally accepted for the structure of such natural high poly-

mers as cellulose. In these investigations a new technic—molecular distillation—was applied to the propagation of chemical reactions.

In this study a method new in principle was developed for the synthesis of many-membered cyclic compounds. A large number of many-membered cyclic compounds was synthesized, including several of entirely new types. Some of these compounds had musk-like odors and are otherwise similar in their properties to the genuine musks. One of these new many-membered ring compounds has found industrial application. The large amount of experimental material made possible important deductions bearing on the relationship between chemical structure and ease of ring formation. His contribution was a major one to the field of many-membered ring compounds, which is one of growing significance in organic chemistry.

He investigated the means by which polymers structurally analogous to cellulose and silk could be prepared, and synthesized a large number. These materials constituted the first completely synthetic fibres with a degree of strength, orientation, and pliability comparable with natural fibres. Their study made possible the development of a theory for the relation between structure, fibrous properties, and other physical properties. The work was brilliant and the most important aid in recent years to the understanding of such polymers. This information, and the modification of the physical and chemical properties of polymers by slight changes in the mode of preparation, has made possible the exploration of a wide variety of substances of most promising industrial application.

Based on this work, a commercial development by the chemists and chemical engineers of the du Pont Company has already resulted. An announcement has just been made (October 28, 1938) that the du Pont Company will erect a plant in Seaford, Delaware, which will cost upwards of eight million dollars, for producing a new textile yarn to be known as nylon. This consists of a synthetic fibre-forming polymeric amide with a protein-like chemical structure, characterized by extreme toughness, strength and peculiar ability to be formed into fibres and into various shapes such as bristles and sheets. Filaments of extreme

fineness can be spun, much finer than the filaments of either silk or rayon. One of the more important uses to which nylon will be put is the manufacture of fine hosiery from high-twist nylon yarn. Hosiery made of the new product possesses extreme sheerness, high elasticity, high strength, and improved resistance to runs. Other uses are sewing thread, knit goods, brush bristles, racquet strings, fishing lines and leaders, narrow fabrics, woven dress goods, velvets, plastic compositions, textile finishing agents, and coated fabrics. Exton bristles, the name given to those made from nylon, have already reached the commercial market in the form of "miracle tuft" tooth brushes.

BIBLIOGRAPHY OF WALLACE HUME CAROTHERS

Catalytic Hydrogenation

Platinum oxide as a catalyst in the reduction of organic compounds. II. Reduction of aldehydes. Activation of the catalyst by the salts of certain metals. (With Roger Adams.) Journ. Amer. Chem. Soc., *45*, 1071-86 (1923).

V. The preparation of primary alcohols by the catalytic hydrogenation of aldehydes. (With Roger Adams.) Journ. Amer. Chem. Soc., *46*, 1675-83 (1924).

VII. A study of the effects of numerous substances on the platinum catalysis of the reduction of benzaldehyde. (With Roger Adams.) Journ. Amer. Chem. Soc., *47*, 1047-63 (1925).

The preparation of some primary amines by the catalytic reduction of nitriles. (With G. A. Jones.) Journ. Amer. Chem. Soc., *47*, 3051-57 (1925).

Valence

The Isosterism of Phenyl Isocyanate and Diazobenzene-Imide. Journ. Amer. Chem. Soc., *45*, 1734-38 (1923).

The double bond. Journ. Amer. Chem. Soc., *46*, 2226-36 (1924).

Reactivity

The reactivities of some tertiary bromides. Journ. Amer. Chem. Soc., *48*, 3192-97 (1926).

The preparation and base strengths of some amines. (With C. F. Bickford, and G. J. Hurwitz.) Journ. Amer. Chem. Soc., *49*, 2908-14 (1927).

Alkali Alkyls

The thermal decompositions of sodium ethyl. (With D. D. Coffman.) Journ. Amer. Chem. Soc., *51*, 588-93 (1929).

The thermal decomposition of sodium and potassium methides. (With D. D. Coffman.) Journ. Amer. Chem. Soc., *52*, 1254-59 (1930).

Polymerization and Ring Formation

Studies of polymerization and ring formation. I. An introduction to the general theory of condensation polymers. Journ. Amer. Chem. Soc., *51*, 2548-59 (1929).

II. Poly-esters. (With J. A. Arvin.) Journ. Amer. Chem. Soc., *51*, 2560-70 (1929).

III. Glycol esters of carbonic acid. (With F. J. Van Natta.) Journ. Amer. Chem. Soc., *52*, 314-26 (1930).

IV. Ethylene succinates. (With G. L. Dorough.) Journ. Amer. Chem. Soc., *52*, 711-21 (1930).

Association polymerization and the properties of adipic anhydride. Journ. Amer. Chem. Soc., *52*, 3470-71 (1930).

V. Glycol esters of oxalic acid. (With J. A. Arvin, and G. L. Dorough.) Journ. Amer. Chem. Soc., *52,* 3292-3300 (1930).

VII. Normal paraffin hydrocarbons and high molecular weight prepared by the action of sodium on decamethylene bromide. (With J. W. Hill, J. E. Kirby, and R. A. Jacobson.) Journ. Amer. Chem. Soc., *52,* 5279-88 (1930).

VIII. Amides from ε-Aminocaproic acid. (With G. J. Berchet.) Journ. Amer. Chem. Soc., *52,* 5289-91 (1930).

IX. Polymerization. Chem. Rev., *8,* 353-426 (1931).

Über die angeblichen Isomerien bei cyclischen Oxalsäureestern. (With F. J. Van Natta.) Ber., *64,* 1755-59 (1931).

Giant molecules and synthetic silk. (With J. W. Hill.) Silk Journal and Rayon World, October 1931. (Also in other technical journals.)

X. The reversible polymerization of six-membered cyclic esters. (With G. L. Dorough, and F. J. Van Natta.) Journ. Amer. Chem. Soc., *54,* 761-72 (1932).

XI. The use of molecular evaporation as a means for propagating chemical reactions. (With J. W. Hill.) Journ. Amer. Chem. Soc., *54,* 1557-59 (1932).

XII. Linear superpolyesters. (With J. W. Hill.) Journ. Amer. Chem. Soc., *54,* 1559-66 (1932).

XIII. Polyamides and mixed polyester-polyamides. (With J. W. Hill.) Journ. Amer. Chem. Soc., *54,* 1566-69 (1932).

XIV. A linear superpolyanhydride and a cyclic dimeric anhydride from sebacic acid. (With J. W. Hill.) Journ. Amer. Chem. Soc., *54,* 1569-79 (1932).

XV. Artificial fibers from synthetic linear condensation superpolymers. (With J. W. Hill.) Journ. Amer. Chem. Soc., *54,* 1579-87 (1932).

XVI. A polyalcohol from decamethylene dimagnesium bromide. (With J. E. Kirby.) Journ. Amer. Chem. Soc., *54,* 1588-90 (1932).

XVIII. Polyesters from ω-Hydroxydecanoic acid. (With F. J. Van Natta.) Journ. Amer. Chem. Soc., *55,* 4714-19 (1933).

XIX. Many-membered cyclic anhydrides. (With J. W. Hill.) Journ. Amer. Chem Soc., *55,* 5023-31 (1933).

XX. Many-membered cyclic esters. (With J. W. Hill.) Journ. Amer. Chem. Soc., *55,* 5031-39 (1933).

XXI. Physical properties of macrocyclic esters and anhydrides. New types of synthetic musks. (With J. W. Hill.) Journ. Amer. Chem. Soc., *55,* 5039-43 (1933).

XXII. Stereochemistry and mechanism in the formation and stability of large rings. (With J. W. Hill.) Journ. Amer. Chem. Soc., *55,* 5043-52 (1933).

XXIII. ε-Capro-Lactone and its polymers. (With F. J. Van Natta, and J. W. Hill.) Journ. Amer. Chem. Soc., *56,* 455-7 (1934).

XXIV. Cyclic and polymeric formulas. (With J. W. Hill.) Journ. Amer. Chem. Soc., *57,* 925-8 (1935).

XXV. Macrocyclic esters. (With E. W. Spanagel.) Journ. Amer. Chem. Soc., *57*, 929-34 (1935).

XXVI. *m*- and *p*-Rings. (With E. W. Spanagel.) Journ. Amer. Chem. Soc., *57*, 935-6 (1935).

XXVII. Preparation of macrocyclic lactones by depolymerization. (With E. W. Spanagel.) Journ. Amer. Chem. Soc., *58*, 654 (1936).

XXVIII. ε-Caprolactone. (With F. J. Van Natta, and J. W. Hill.) Journ. Amer. Chem. Soc., *58*, 183 (1936).

XXIX. Polymers and polyfunctionality. Trans. Faraday Soc., *32*, 39 (1936).

Acetylene Derivatives and Synthetic Rubber

Acetylene polymers and their derivatives. II. A new synthetic rubber; chloroprene and its polymers. (With Ira Williams, A. M. Collins and J. E. Kirby.) Journ. Amer. Chem. Soc., *53*, 4203-25 (1931).

Homologs of chloroprene and their polymers (Second paper on new synthetic rubbers). (With D. D. Coffman.) Journ. Amer. Chem. Soc., *54*, 4071-76 (1932).

III. The addition of hydrogen chloride to vinylacetylene. (With G. J. Berchet, and A. M. Collins.) Journ. Amer. Chem. Soc., *54*, 4066-70 (1932).

IV. The addition of hydrogen bromide to vinylacetylene, bromoprene and dibromobutene. (With A. M. Collins, and J. E. Kirby.) Journ. Amer. Chem Soc., *55*, 786-88 (1933).

V. The polymerization of bromoprene (Third paper on new synthetic rubbers). (With J. E. Kirby, and A. M. Collins.) Journ. Amer. Chem. Soc., *55*, 789-95 (1933).

VI. Vinylethinylmagnesium bromide and some of its reactions. (With G. J. Berchet.) Journ. Amer. Chem. Soc., *55*, 1094-96 (1933).

VII. Sodium vinylacetylide and vinylethinylcarbinols. (With R. A. Jacobson.) Journ. Amer. Chem. Soc., *55*, 1097-1101 (1933).

VIII. α-Alkyl-β-Vinylacetylenes. (With R. A. Jacobson.) Journ. Amer. Chem. Soc., *55*, 1622-24 (1933).

IX. 1-Alkyl-2-chloro-1, 3-butadienes and their polymers (Fourth paper on new synthetic rubbers). (With R. A. Jacobson.) Journ. Amer. Chem. Soc., *55*, 1624-27 (1933).

X. The chlorination of the hydrochlorides of vinylacetylene. (With G. J. Berchet.) Journ. Amer. Chem. Soc., *55*, 1628-31 (1933).

XI. Dichloro-2,2-butadiene-1,3 and trichloro-1,2,3-butadiene-1,3. (With G. J. Berchet.) Journ. Amer. Chem. Soc., 55, 2004-08 (1933).

XII. The addition of the thio-*p*-cresol to divinylacetylene. Journ. Amer. Chem. Soc., *55*, 2008-12 (1933).

XIII. The action of chlorine on divinylacetylene. (With D. D. Coffman.) Journ. Amer. Chem. Soc., *55*, 2040-47 (1933).

XIV. The dihydrochloride of divinylacetylene. (With D. D. Coffman, and J. A. Nieuwland.) Journ. Amer. Chem. Soc., *55*, 2048-51 (1933).

XV. Halogen-4-butadienes-1, 2. The mechanism of 1, 4-addition and of α, γ-rearrangement. (With G. J. Berchet.) Journ. Amer. Chem Soc., *55*, 2807-13 (1933).
XVI. The preparation of orthoprenes by the action of Grignard reagents of chloro-4-butadiene-1, 2. (With G. J. Berchet.) Journ. Amer. Chem. Soc., *55*, 2813-17 (1933).
XVII. Mercury derivatives of vinylacetylene. (With R. A. Jacobson, and G. J. Berchet.) Journ. Amer. Chem. Soc., *55*, 4665-67 (1933).
XVIII. 1-Halogen-2-vinylacetylenes. (With R. A. Jacobson.) Journ. Amer. Chem. Soc., *55*, 4667-69 (1933).
The synthetic rubber problem. Ind. and Eng. Chem., Anal. Ed., *6*, 56-59 (1934).
Structure of divinylacetylene polymers. (With M. E. Cupery.) Journ. Amer. Chem. Soc., *56*, 1167-69 (1934).
Addition of alcohols to vinyl acetylene. (With R. A. Jacobson, and H. B. Dykstra.) Journ. Amer. Chem. Soc., *56*, 1169 (1934).

Organic syntheses

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1936497 Fermentation glycerol.
(With F. J. L. Van Natta)
1946258 Organic acids such as acetic acid.
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1950431 Addition products from monovinyl acetylene and hydrogen halide.
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1950432 Polymers such as those of 2-chloro-1, 3-butadiene.
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1950433 2-Bromo-1, 3-butadiene.
1950438 Polymerizing 2-halo-1, 3-butadienes.
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1950439 Polymerizing 2-halo-1, 3-butadienes.
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1950441 Halobutadienes.
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1963074 Vinylethinyl carbinol polymers.
(With G. J. Berchet and R. A. Jacobson)
1963934 Vinylethinyl derivatives.
(With R. A. Jacobson)
1963935 Vinylethinyl derivatives.
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1965369 Di- and tri-chloro-1, 3-butadienes and their polymers, etc.
(With G. J. Berchet)

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1967860 Rubber-like polymerization products of 2-chloro-1, 3-butadiene. (With A. M. Collins and J. E. Kirby)

1967862 Cyclic compounds prepared from β-substituted-α, γ-dienes, etc. (With A. M. Collins)

1995291 Trimethylene carbonate, etc.

1998442 Dichlorobutadiene. (With G. J. Berchet)

2008003 Alkylated lead phenolates.

2012267 Esters of dibasic acids.

2013725 Alcohol addition products of unsaturated compounds such as mono- or divinyl acetylene. (With R. A. Jacobson)

2019118 Laminated glass ("safety glass"). (With G. J. Berchet and R. A. Jacobson)

2020298 Formation of cyclic esters by depolymerizing the corresponding linear polyester. (With J. W. Hill)

2029410 Polymerization products suitable for molded articles, coatings, etc. (With A. M. Collins and J. E. Kirby)

2038538 2-Chloro-1, 3-butadiene, etc.

2061018 Hydrogen polysulfide addition products of unsaturated aliphatic hydrocarbons (resinous products suitable for use in film-forming compositions).

2066329 Polymerizing compounds such as 2-chloro-1, 3-butadiene. (With A. M. Collins and J. E. Kirby)

2066330 Polymerizing 2-chloro-1, 3-butadiene in the presence of vinyl compounds to form rubber-like polymers. (With A. M. Collins and J. E. Kirby)

2066331 Polymerizing 2-chloro-1, 3-butadiene in the presence of α, β-unsaturated aldehydes, ketones, esters, anhydrides, nitriles, etc. (With A. M. Collins and J. E. Kirby)

2067172 Chlorination products of polymerized 2-chloro-1, 3-butadiene, etc.

2068265 Vapor-phase production of aliphatic organic acids. (With G. B. Carpenter)

2071250 Linear condensation "superpolymers" suitable for production of pliable, strong elastic fibers.

2071251 Artificial synthetic fibers from synthetic linear condensation superpolymers.

2071252 Synthetic linear polyacetals by condensation between a glycol and an acetal of a monohydric alcohol.

2071253 Polyamides from amino acids.

2072867 Synthetic rubber and initial materials for its production.

2073363 Butadienyl compounds. (With G. J. Berchet)

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2082568	Vinylacetylene derivatives. (With G. J. Berchet)
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2102611	Dichlorobutene and its preparation. (With A. M. Collins)
2104789	Isomerization of isohaloprenes.
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2136177	Reaction of chloro-4-butadiene-1, 2 with amines. (With G. J. Berchet)
2136178	Reaction of chloro-4-butadiene-1, 2 with alkaline reacting metal inorganic compounds. (With G. J. Berchet)
2137235	Shaped articles from polymeric materials.

Several other patents will be issued to Doctor Carothers during the next two years.